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I.—EMPIRICAL PROPOSITIONS AND  
HYPOTHETICAL STATEMENTS

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IT is becoming the fashion among empiricist philosophers to assume that phenomenism is really dead at last. Provoked into existence by non-naturalistic notions of material substance, it successfully undermined them; but it shared a sufficient number of fundamental metaphysical assumptions with its defeated rival to perish with it when the system of thought which nourished both was destroyed, in the very act of victory. A better ontology than that of Descartes or Locke, but still an ontology, it is therefore now held to be obsolete; and doubtless this is how it ought to be. But if phenomenism is dead, the memory of it still haunts the writings of modern discussions of the nature of the external world to a surprising degree; from Eddington's notorious two desks, to the more refined and penetrating analysis of better equipped philosophical authors, it makes its presence clearly felt, usually taking the form of a sharp distinction; now between observation statements and those concerning material objects; now between two or more senses of the verb "to see"; at other times between 'basic' or 'protocol' sentences and those of ordinary speech; or between various 'modes' of speech; or between "strong" and "weak" verification. Such versions of it are almost always formally guaranteed to carry no "metaphysical" implications; nevertheless their striking resemblance to the older discredited variety is hard to overlook. Hence, an examination of its latest manifestations is not such a flogging of a dead horse as at first it may seem to be; for if it is dead, its ghost walks, and should, if possible, be laid.

Two further assumptions are made in the course of the following remarks :

(i) The argument against the phenomenalist analysis of common-sense statements leaves open the question whether the information provided by the exact sciences such as physics, can be translated without loss into phenomenalist terms. Perhaps it can ; and perhaps this demonstrates something of importance ; it has always been considered that the language of science could, with no alteration of its "meaning", be translated into solipsistic terms ; which, however, is not held to be an argument in favour of solipsism. But if such a "translation" does not adequately render the empirical descriptions of ordinary language, this will affect the propositions of science only in so far as these claim to be an extension of ordinary language used to describe the world, and not a specialised method of referring to aspects of it for some narrower, predictive or other, purposes—a specialised use of words which may be susceptible to a phenomenalist analysis. In any case the answer to the question whether this is so is, I believe, logically independent of the rest of my argument.

(ii) Nor do I wish to deny the historical achievement of phenomenism. Whatever its defects—and I shall wish to say that they are fatal—it has made less excusable any return to those ancient delusions which the philosophers of substance from Thales to G. F. Stout have done much to promote. But beneficent as its influence has been, it has overstayed its welcome ; its continued presence does more harm than good ; and the argument set out below is intended to provide additional reasons for consigning it finally to an honoured grave.

## I

Many forms of modern empiricism, and in particular modern phenomenism, rest on the view that expressions describing material objects must in principle be capable of being translated (without residue) into sets of sentences about the data of actual or possible direct sensible acquaintance, past, present and future, on the part of real or possible observers ; (sensible is here used in the widest sense—to cover all states, activities or dispositions capable of being studied by empirical methods). Any alternative theories of how material object propositions are to be analysed tend to be rejected out of hand by modern empiricists on the ground that this must at some stage involve belief in the existence of non-sensible or transcendent entities or characteristics, and this is ruled out for the familiar reasons

advanced, for example, by Berkeley, and rests on his theory of words ; according to this, no expressions purporting to describe material objects can have any meaning, let alone be true, unless all the entities or characteristics to which they refer are either found in sensible experience—in the sense of “sensible” defined above—or can be analysed into entities or characteristics so found. Since most empiricists hold that any alternative analysis of material object propositions involves the possibility of acquaintance with non-sensible entities or characteristics—and this they hold to be an unintelligible suggestion—phenomenalism appears to follow automatically. Disagreement can arise only about the adequacy of this or that suggested analysis of how material object sentences are to be “reduced” (without residue) to sentences describing both what the observer does, or did, or will observe, as well as what he would, or would have, might or might have, observed under appropriate conditions ; and the provision of alternative analyses on these lines has taxed the ingenuity of some of the acutest philosophers of our day. But common sense and the philosophers who are in sympathy with it, have always felt dissatisfied. The reduction of material object sentences into what we may, for short, call sense datum sentences, seemed to leave something out, to substitute something intermittent and attenuated for something solid and continuous. To dispel this sense of discomfort, phenomenalists began to explain that it was due to a confusion : the view that they were advocating was neither a metaphysical nor a scientific theory of what things were made of, or how they behaved, but something less adventurous—no more than an alternative language capable of rendering all that could be described in the material object language, and recommended for its therapeutic properties as an antidote to metaphysical hankering after non-sensible substitutes. If translation into the sense datum language still seemed to leave something out—what some philosophers have called the ‘irreducible categorical element’ of material object propositions, this missing element was labelled emotive—a psychological residue—with no descriptive function ; or else it was (with somewhat greater insight) connected with the legitimate demand for the kind and degree of vagueness, indefiniteness, and rich ambiguity of speech needed by the plain man for his normal, every day purposes. But it was claimed that at any rate the hard core of descriptive meaning could be successfully transplanted, as it were, into the new language. The phenomenalist equivalent of a material object sentence might, like a new shoe, seem uncomfortable at first, but continued use

would presently dissipate this feeling. The discomfort was only "psychological", due to linguistic habits harmless in themselves, but tempting philosophers to false doctrines about both language and the world.

Common sense continued to experience a certain discomfort, but found it difficult to formulate it in words. Professor G. F. Stout<sup>1</sup> complained that the opaqueness—"the permanent impossibility of sensation" of material objects had been unjustifiably eliminated. Mr. W. F. R. Hardie<sup>2</sup> found it puzzling that 'hypothetical' causes could be said to cause 'actual' effects—but this was held, e.g. by Professor A. J. Ayer<sup>3</sup> to be mainly due to a misunderstanding of the language which phenomenalists were trying to use or "recommend". What I propose to do is to try and articulate what the main source of the discomfort felt by common sense seems to me to be, since I think that in this case the doctor's diagnosis too often neglects the specific nature of the patient's complaint. For it seems to me to be more than a mere source of discomfort, namely a valid and fatal objection, to the phenomenalist analysis. However, even if I am mistaken in this, the complaint itself still seems worth examining.

It may be worth adding that even if phenomenalism turns out to be unacceptable, some of the stock objections to it are not less so. For the familiar anti-phenomenalist theses are often, even when valid, formulated in such a way as to convey anxiety to salvage altogether too much from the ruins of the theory they are intended to destroy. Consider, for example, the four most familiar types of attack upon it.

(1) One of the most familiar objections urged against, for example, Berkeley, or Mill or Russell, is that when converting sentences about material objects into sentences about sense data, they fail to 'convert the observer' who 'occurs' in the protasis of the hypothetical statement, into 'sense data'—he remains irreducibly 'material'. It has indeed been suggested<sup>4</sup> that to 'dissolve the observer' a second proposition could be constructed, which presumably, would describe the activities of a second 'observer' who actually or potentially observes

<sup>1</sup> *Studies in Philosophy and Psychology*, pp. 136-37.

<sup>2</sup> 'The Paradox of Phenomenalism', *Proceedings of the Aristotelian Society*, 1945-46.

<sup>3</sup> 'Phenomenalism', P.A.S., 1946-47.

<sup>4</sup> This argument was first developed to the best of my belief by Professor H. H. Price. A somewhat more complicated method of the progressive "elimination" of material bodies is propounded by Mr. R. B. Braithwaite ("Propositions about Material Objects" P.A.S., 1937-38).

the body of the original 'observer'; this 'observer' in his turn requires a third 'observer' to observe him; and so we should get a Chinese box series of possible observers—referred to by a logically similar series of propositions, which would progressively 'reduce' or 'dissolve' the residual material object content of the original protasis. This asymptotic process of gradual whittling would tend to the ideal limit of pure phenomenism. Then by somehow integrating the series, one might represent the material object as definable in terms of it. A criticism related to the original objection is that such ideal 'observers' and their behaviour could not be properly described without perpetual reference to material objects, *e.g.* those which determine 'their' position in space, movements, etc. Each of which again, for its analysis, at every point presupposes yet other material objects, so that the attempted analysis cannot get going without breaking down at any and every point in the process. Some philosophers try to soften the force of this objection by saying that such theoretically infinite theories have pragmatic limits set by the context and the practical needs of the situation and sometimes<sup>1</sup> hold that sufficiently painstaking analysis (and most analysts are too lazy or bored to do the plodding required) could go a long way towards achieving pure phenomenism. What both these kinds of objection, whether they are valid or not, suggest is that if phenomenism fails, it very nearly achieves its result—the unresolved residue can be got down to almost vanishing point—which is perhaps as much as one can reasonably hope for.

(2) Another often heard objection is that the hypothetical propositions about the experiences of observers which are indispensable to the phenomenalist analysis, seem to involve something like the existence or reality of 'hypothetical facts' or 'hypothetical sense data', or 'unsensed sensibilia'. For otherwise, what do hypotheticals describe? Surely not nothing? And these postulated entities, unknown and unknowable to science and common sense, are, so it is urged, at least as mythological as the Lockean substratum which they were invoked to exorcise. Phenomenism is accused of breeding new metaphysical entities—with their own pseudo-problems: but if we could only get rid of these somehow, say by an improved, non-correspondence theory of meaning, all might still be well.

(3) It has also frequently been asserted that the promised 'reduction' of common-sense language by such methods as those of Descriptions, Logical Constructions, etc., cannot in fact be performed successfully. Phenomenalists are challenged to

<sup>1</sup> *Vide* Mr. D. G. C. Macnabb ("Phenomenism", P.A.S. 1940-41).

provide an equivalent in sense datum currency of propositions about material objects, and when they decline to produce the precise equivalent, they are accused of uttering counterfeit cheques : and this is said to hold even more obviously of scientific entities—the promise to construct “many storied”<sup>1</sup> logical constructions, with sense data as foundations, and gamma particles two or three floors above—has not been kept. Phenomenalists are accused of maintaining that, although phenomenalist language might be intolerably clumsy and prolix, it could in principle always be substituted for the ellipses of common speech : that normal language has the character it has in order to serve the use that it serves ; that sense datum language would doubtlessly be inconveniently precise and definite and intolerably lengthy and tedious, and would have its own unfamiliar “grammar”, but that in principle the translation could be effected, although by sacrificing so much customary vagueness, ambiguity, indefiniteness, etc., as would render it useless for every day purposes. Against this, the opposition maintains that it is only necessary to try and put this programme into practice to see that it is a labour of Sisyphus and will not work : vagueness, ambiguity, etc., are inalienable properties of common-sense language ; but for this, the programme could perhaps be carried out ; but as it is, the claim to reduce—plausible enough *prima facie*—turns out to be hollow once the bluff is called. Yet the reason for this is still the comparatively weak one that we should lose too much in the way of nuances, range, implied meanings of words ; the feeling remains that the “hard core” of meaning might still be “reduced” or translated.

(4) Finally, there are the difficulties about dealing with propositions about other minds, communication, etc., in the appropriate Humean manner, too familiar to be repeated ; which theoretically leaves open the possibility of the programme advanced by Berkeley whereby phenomenalism works for material objects and breaks down only in the case of persons.

The above is a characteristic selection from the, by now traditional, array of anti-phenomenalist arguments. I should like to suggest that, formidable and indeed fatal as some of them may be, they are usually so formulated as to convey a misleading impression, for despite their anti-phenomenalist air they are all in effect so much concealed pro-phenomenalist propaganda. The suggestion implicit in all these criticisms is that, while the phenomenalist goal is and must be striven towards—for the alternative is a metaphysical morass—the particular

<sup>1</sup> *Vide* Mr. D. G. C. Macnabb (*loc. cit.*).

avenues thus far offered by phenomenalists are unfortunately blocked by various types of logical or epistemological obstacles : in other words that some such operation is desperately needed if we are ever to eliminate unverifiable or indescribable entities, but that the techniques offered by various philosophers have all, so far, broken down. This position is not unlike the situation with regard to say Fermat's theorem : what the theorem asserts is considered as being very likely true, at any rate not demonstrably fallacious, and in any case as being the kind of assertion which should be capable of demonstration or refutation by normal mathematical techniques. Similarly all phenomenalist operations so far conducted have indeed ended in failure ; but they, and only they, are the kind of processes which can, in principle, be applied. *Some* kind of phenomenalist analysis must be correct, for the only alternative is a return to Locke, or Descartes, or Kant, and that, in this enlightened age, is surely not a thinkable course.

This is the bogey used to drive philosophers back to make yet another gallant attempt to break out of the impasse—to find a 'viable' translation into the sense datum language. The impression conveyed throughout, possibly because of a faulty theory of meaning and truth, is that phenomenalism is, after all, the only possible valid view, beset though it may be by grave objections and exaggerations : the problem is one of technical skill : once it is reformulated with sufficient ingenuity the problem will be solved, or dissolved. My thesis is that phenomenalism is not even *prima facie* plausible—let alone indispensable, and minor improvements, *i.e.* tinkering, cannot make it more so. Instead, therefore, of re-examining the all too familiar current objections to phenomenalism, and the answers to them, I should like to suggest that it might be valuable to try to find out what it is that makes common sense so uncomfortable—in order to see whether this discomfort is merely "psychological", and perhaps due to the relatively accidental properties of ordinary language, or whether it is a symptom of some fatal defect in the theory.

## II

What common sense, from Dr. Johnson onwards, finds paradoxical in all phenomenalist analyses, is, I believe, this : I say, "There is a brown table in the next room." This, I am told, should mean a set or range of propositions of the type, "If 'a normal observer' were to go next door and look, he would, in

normal light, other normal conditions, etc., see such and such brown-coloured data, etc." I say, "But supposing no one goes next door, what have we then? Is the apodosis false? Are there no brown data and no table?" I am told, "Of course not. Nothing regarding the consequent follows from denying the antecedent. In a sense, nothing follows at all. It still remains true that if someone looked, etc., he *would* see brown data, etc." I accept all this and remain dissatisfied. If I believe that there was in pre-historical times a land bridge between Africa and America, then I agree—and possibly this is analytic—that if there had been an observer at that time suitably placed, he would have seen the land bridge or a portion of it. But I may wish to assert that, in fact, no such observer existed, and that the land bridge was nevertheless there, whether or not this is true. What I think common sense and Professor G. F. Stout wish to say is that the question of the existence of the land bridge, like the existence of the table next door, is one thing, and the question of the presence or absence, even hypothetically, of an observer, is another. The statement that if there had been (and there was not), any observer, he would have observed (and no one did observe), certain data, seems to them not equivalent to asserting the past existence of material objects. Categorical propositions about material objects are replaced by unfulfilled "counterfactual" hypothetical propositions about observers, and what troubles the plain man is the thought that if the hypotheticals are unfulfilled, if no observers were in fact observing, then if the phenomenalist analysis is correct, there was—in a sense datum sense—nothing at all, and, moreover, that this sense of 'existence' is basic: because the alleged material object sense in which the non-existence of actual sense data nevertheless can be 'translated into' the existence of material objects, is not a sense in which the word 'exist' is commonly understood. If he is then told that to say there was a material object—the land bridge in pre-historic times—is to say something about data there would have been if . . . he feels cheated. For these data appear to depend on the activity of observers; so that the material object becomes analysed into a series of either purely hypothetical, *i.e.* non-existent, or at best, intermittent data occurring and disappearing as the observer observes and ceases to observe. And this seems empirically a different picture of the world from that which he started by believing; and in no sense merely a description of the old picture though in different words.

I shall try to make this clearer. To analyse material objects in terms of the hypothetical data of observers is, in

effect, to turn the statements about them into statements about the dispositional characteristics of observers. "The table next door exists", on this view means that you or I or X, who are in this room, are possible or potential table-data observers. This asserts the existence of a dispositional characteristic; but dispositional characteristics are so called in order to contrast them with non-dispositional characteristics, the "grammar" of which is rightly said to be different. If I ask, "Does he look much the same when he is asleep?" that it a plain, empirical question, the answer to which can be discovered by ordinary empirical means, *i.e.* by looking. But if I ask, "Is he clever even when he is asleep?" this sounds quite wrong—I am rightly told that I evidently do not understand how the word "clever" is commonly used; surely, I am told, to say that someone is clever is to say something of the following sort: that if certain sorts of questions are put to him, he will easily and correctly answer them, or that he grasps certain types of data and makes inferences from them more successfully than most people, and so on. When he is asleep, these conditions do not occur and the question is therefore inappropriate to the situation. How does all this apply to the table next door? The assertion that there is a table next door is made equivalent to what the observer would see if he looked, etc., *i.e.* a collection of hypothetical, *i.e.* dispositional-causal propositions about the observer; but when the causes do not materialise, neither, as a rule, do their effects, and when neither exists, there is a gap in the series of sense datum events. We accept this quite naturally in the case of normal dispositional characteristics: "X is irritable" is compatible with, indeed it is compatible only with, "He flies into tempers on slight provocation, or sometimes when there is no provocation at all," *i.e.* at other times there are no bursts of temper, no continuing real substratum—there does not literally exist, in the ordinary sense of "exist", something called potential irritation going on like volcanic activity underneath the surface; we do speak of unconscious or suppressed irritation, but to take this literally is to confuse words with things, to confuse the mythology of psycho-analysis with the furniture of the real world, to fall into Locke's errors. But if I say, "The table is next door (or 'the table has a back to it,' or 'the table was here two hours ago') even with no one looking," do I mean, "There are table-data whenever people look; but at other times, when no one is looking, nothing at all?" This is precisely what common sense does *not* believe to be true about tables. Common sense endows them with 'actual', *i.e.* non-dispositional

characteristics in the absence of observers. The table is seen intermittently or not at all: the intermittent presence or non-existence of observers is a part of the intermittent or unrealised series of causes or conditions of its being seen; but it—the table—is assumed to have some characteristics continuously; it differs from irritability precisely in this respect—that unlike irritability it is believed to exist continuously in the literal sense when there are no intermittent data, no glances directed at the table. To analyse material object statements as statements about dispositional characteristics of observers, therefore, is to represent the material object as being, at most, an intermittent series of actual data with the gaps filled by hypothetical "non-actualised" entities, *i.e.* in the sense datum sense, by nothing at all. This, for common sense, is tantamount to destroying the continuity of the table—its history before and after it is observed, its unseen portion, its presence next door. Of course, phenomenologists stoutly and indignantly denounce this conclusion as a confusion of two senses of 'existence', a crude misunderstanding of the very notion of logical constructions. Tables, we might be answered, are logical constructions as irritability is: in both cases, the essential task is to eliminate Locke's substratum and to substitute for it a set of intermittent and hypothetical data. The unobserved table, or its unobserved back, continue to be as someone's irritability continues to be. Yet common sense does not raise difficulties of this type about the analysis of irritability; it accepts easily enough that irritability does not exist in the same sense as an actual burst of temper is said to do, that to speak of irritability is to use a kind of shorthand for a complex of causal laws and observation propositions. But when I say, "There exists a table such as you describe", am I really saying that it exists in the same sense of 'exists' as someone's irritable temper? Some characteristics of tables may, of course, genuinely be described as dispositional, *i.e.* in speaking of them, I am referring to certain causal laws and hypothetical or intermittent data—*e.g.* when I say a table is combustible or useful or expensive. But this only means something by contrast with those properties of the table which are not dispositional, and perhaps, a good many intermediate properties which we do not think of either as definitely dispositional or definitely 'actual'. The suggestion that *every* characteristic of the table is merely possible or intermittent or depends on dispositions of observers—that *everything* is dispositional, nothing actual—is exactly what common sense and Dr. Johnson revolt against, not as being untrue, but as coming close to being meaningless, and

certainly as suspiciously approaching some kind of solipsism—and one not very easy to describe in empirical (or any other intelligible) language.

What common sense dislikes is precisely the crucial role played by hypotheticals in the phenomenalist analysis, and it seems to me to display a sound instinct in so doing.

For this is the central point of this entire issue: that the translation of categorical existential statements into hypotheticals (of whatever 'level') is a dangerous operation and cannot be left to the mechanical operation of 'syntactical' rules because different types of sentence do have certain normal uses in ordinary language—at any rate in most modern European languages—which we ignore at our peril; Humpty-Dumpty's nominalism goes too far: words are sometimes masters if we are to communicate without perpetual recourse to redefinition *i.e.* if we are to communicate at all; and as we use words, categorical sentences, on the whole, tend to convey that the object referred to has occurred or is occurring or will occur in time; existed, is in existence, will exist; they have a non-descriptive, existential, ostensive element; they seem to invite us to look for the entity they purport to be about, and only when there is none such in any normal sense, *e.g.* in the case of a sentence like, "Bad temper is unattractive", do we avoid pseudo-problems by turning to the hypothetical mode of expression as the more natural, as likely to elucidate what is being asserted in words better adapted to expressing it. Existential propositions expressed categorically—in indicative sentences—tend, as it were, to "point" towards their "objects"; and demonstratives which appear in existential propositions, like, "this is", "there is", "here we have", etc., often function as substitutes for such acts of pointing to things or persons or processes. The characteristic force of the categorical mode of expression is often exactly this—that it acts in lieu of a gesture, of an 'act of ostension', "Here is the book", I say to someone looking for it, or I could point to it and say, "The book", and convey roughly the same information by both methods. But hypotheticals normally do the opposite of this. Hypotheticals, whatever they describe or mean, whatever they entail or convey or evince, in whatever way they are verified or fail to be verified, do *not* as a general rule directly assert that something has been, is being, or will be occurring, or existing, or being characterised in some way: this is precisely the force of the conditional mood, and it is realisation of this which probably led Ramsey, for example, to assert that causal propositions were not descriptive at all, but commands or rules.

Ramsey's analysis can easily be shown to be unsatisfactory, since it seems to rest on a fatally false view of the nature of meaning ; but the feeling which led him to so strong a separation of general and hypothetical forms of expression from, say, singular categorical sentences, did not altogether lead him astray. For this gulf does divide categoricals and hypotheticals in our normal usage : whereas the first is normally used to describe the furniture of the world—what is, was or will be—the second is not ; consequently, whenever a categorical (indicative) form of expression is used, often quite idiomatically, to convey something other than what is, or was, or will be, it is easily and without resistance on the part of common sense, replaceable by a hypothetical (conditional) sentence—as in the case of indicative sentences referring directly or indirectly to dispositions, or general propositions of the “all, every, any” type. But even this is in need of a significant qualification. If the general terms are so used as to suggest that they possess extension of any kind, the hypothetical form is felt to be to that extent insufficient, and categorical expressions are required to complete the analysis. Thus, “Anyone who was there at three o'clock saw the meteor fall”, because it is compatible with, “And no one in fact was”, can be translated into, “If anyone was there, or had been there, etc., then he saw, or would have seen, etc.” ; whereas, “He gave away his books to anyone who asked for them”, is not equivalent to, “If anyone asked for, had asked for, etc., his books he was, or would have been given, etc.”, but needs in addition, “and some persons did ask”. It seems quite clear that in this last instance a conditional or hypothetical sentence by itself tells us nothing about what in fact happened, and an indicative or categorical one is therefore required by ordinary usage to convey “existential import”—to refer to actual events which are believed to have taken place.

All this may seem altogether too trite and obvious, but there is a corollary which is evidently less obvious, namely, that no direct translation from categoricals into hypotheticals is, as a general rule, and as our language is to-day ordinarily used, a correct analysis of, or substitute for them. And this seems to me to destroy one of the indispensable foundations of phenomenalism. For it is this sense of the illicit substitution of hypotheticals for categoricals which is responsible for the obscure feeling on the part of common sense that something—an *ersatz* entity—is being palmed off upon it by phenomenalists. Such a categorical existential material object sentence as, “The table is next door”, or “There is a table next door”, is used at the very least to

describe something which is occurring or being characterised at the time of speaking, together (perhaps) with some sort of prediction (and what has been called retrodiction) about what has been or will be occurring or being characterised during unspecified periods of time before and after the period of speaking; and being characterised or occurring, unless the contrary is specifically stated or implied, not intermittently but continuously, and in any case not 'hypothetically'. For to say that something is occurring hypothetically is a very artificial and misleading way of saying that it is not, in the ordinary sense, occurring at all, but might or would occur if conditions were realised which in their turn may or may not be realised. Consequently, whatever common sense may mean by the sentence, "There is a table next door", it cannot accept as fully equivalent in meaning any sentence not asserting that something is now, or has been, or will be, occurring or being characterised. It may well be that categoricals systematically entail corresponding hypotheticals (or disjunctive sets of such)—that the proposition, "The table is next door now" in some sense entails that if either observer A or observer B or C, etc. were to go next door, one or other of them could see or touch such and such data : for invisible or intangible tables are not what we normally mean by 'table'. Likewise, it may be that hypotheticals in some cases may be said either to entail, or else to state conditions for the truth of, or else 'sufficiently justify' the assertion of categoricals ; in other words, that if it is true that a normal observer (*i.e.* one free from hallucinations, etc.), sees, or has seen, or will see, or would see, or would have seen, certain data, under the appropriate conditions, it follows deductively and not inductively that there is a table next door. Something like this may be correct, and perhaps this is all that the phenomenalist requires as against Locke's insensible substance, or attenuated versions of it, such as 'physical occupants'.<sup>1</sup> For it is clear that if I am to explain under what circumstances I *should* normally assert material object sentences, I can do so only by invoking hypothetical observers and their cognitive states : if I am called upon to describe the conditions in which such and such sentences are appropriate, then I cannot fail to make use of hypotheticals. But to describe

<sup>1</sup> And this is, without doubt, the great historical service of phenomenism—that for more than two centuries it has been pressing home the paradoxical consequences of simultaneously holding both that material objects, if they exist, "must" possess certain characteristics (although no one has been able to identify them at all clearly) which cannot, in principle, be empirically observed, and that these are among the characteristics with which the natural sciences necessarily deal.

conditions in which alone I should be inclined to enunciate a sentence is certainly not equivalent to giving its meaning. For my point is that the hypothetical sense datum sentence cannot be equivalent to, or an analysis of, a material object sentence if the hypothetical (sense datum) sentence asserts only what would be, while the material object sentence sometimes asserts what occurs, occurred, or will occur in the world. Existential propositions about material objects assert what is, was or will be, and not what might be. Stout had every reason to be suspicious of the description of the material world in such dubious terms as, "The permanent possibility of sensation", because however modified and refined, it both suggests a kind of permanent grid-like world framework and denies it. Dr. Johnson's well known attitude does not, after all, rest on such a very gross misunderstanding. That is the heart of the case against phenomenalism.

But what precisely, it may be asked, is it that such categorical existential sentences do that hypothetical ones fail to do? Certainly I wish to avoid saying that the former describe the facts while the latter do not, since the unhappy term "fact" has been used in too many different senses to be illuminating in this connexion. Nor do I wish to assert that hypotheticals and categoricals are never interchangeable and are mutually exclusive—as if the forms of propositions could be distinguished into natural kinds corresponding to 'ontological' or Kantian categories, or 'ultimate grooves in reality'. But I do suggest that systematic differences in verbal form are often pointers to differences in meaning which it is important not to obscure. Hence, as a tentative way of putting it, I submit that those categorical propositions which we seem to be unable to 'reduce' to other logical forms without doing apparent violence to normal usage, tend to direct attention to—invite us to look for—things and events in a way in which other kinds of expressions do not. This is felt most clearly about expressions containing demonstratives like 'this', or 'that', or 'here', but applies no less to existential propositions without demonstratives which identify something in the time series. In the case of objects with some or all of which we claim to be acquainted by some kind of direct inspection, this relation—which for want of a better word I propose to call 'pointing'—can literally occur: in declaring that a particular table is here before me, a particular sound is now growing louder, a particular doubt is now tormenting me, I am pointing at, directing your attention to, something with which I am directly acquainted, an event or

a thing. But if I say, "The table is next door", "The cupboard has a wooden back which you cannot see", "Napoleon wore a three cornered hat", "Napoleon felt a twinge of remorse before the battle", I cannot, of course, in the literal sense be said to be acquainted with, or point at, a thing or event, for it is, in the ordinary sense of the words, not present, not here, not before me, not within my ken. And this is perhaps what lends such plausibility as it seems to have to the phenomenalist procedure of offering me hypotheticals intended both to describe unobserved characteristics and to indicate methods of observing, *i.e.* in some sense verifying them. But this will not do, for whereas the difference between categoricals and hypotheticals is one of logical form, whether syntactical or semantic, the difference between being able and not being able to observe a given object is empirical or causal. I cannot point to the table next door, or at a point beneath its surface, because it is invisible: there is the intervening wall or surface which makes this act unhelpful. In saying "There is a table next door", I am, as it were, trying to refer to the table "through the wall" or to the back or inside of the table as if it were not concealed but before me, in my sense field. If the wall becomes transparent the relevant difference between the table here, in front of me, and the further table next door disappears, for the only relevant difference between the two types of case is that I was originally in a better position in space (or time) to describe the table in front of me. There may be important semantic differences, *e.g.* in learning the use of symbols for present, as opposed to absent, entities,<sup>1</sup> but there is no logical difference between dividing sentences which describe things in my field of vision from those which describe things beyond the horizon.

The kind of communication which a demonstrative, categorical sentence, which purports to be true, seeks to perform in respect of unobserved objects and events, may fail to achieve its object in at least one of two ways: the entity may not exist or possess the characteristics in terms of which it is denoted; or the failure may be due to some defect in my technique—if the relevant entity is not, for whatever reason, recognised by my audience; my effort to communicate is thwarted, but only by such empirical circumstances as physical walls, or the shape of the earth, or the limitations of my senses or imagination, or the date of my birth; thwarted by that and not by something incurably hypothetical, non-existent about the sentence itself.

<sup>1</sup> I owe this point to Dr. F. Waismann.

Let me give an example: when I say that Napoleon wore a three-cornered hat, or that on the evening before the battle of Borodino he had a twinge of remorse, I do not mean (though this is not strictly relevant to the argument) that one man and one man only was called Napoleon, and who ever was so called wore a three-cornered hat, or had a twinge of remorse. Proper names are not usually mere definite descriptions. My use of the word "Napoleon" is, among other things, a substitute for a wave of the hand, an inclination of the head, etc. because I cannot point in a literal sense, if only because I was born too late; and this is ultimately an empirical obstacle like the wall of a room or the nature of light or the structure of my brain. I am inviting you to direct your attention to Napoleon or to physical or mental events in his history and there is a non-descriptive and existential force in my use of the relevant words—and in particular of proper names—because I suggest or believe or know that such events have happened—that they are part of the collection of what was and is and will be. Certain types of categorical sentences in this way direct attention to things and events which therefore are taken to exist whether or not they are observed. The fact that they are in some sense capable of being directly observed, or verified, or their existence supported by sense datum evidence, may be part of the meaning of such concepts as "thing" or "event", but it is not what is asserted when I say that they occur here or now, or have such and such characteristics; and the reason for this is that the hypotheticals which I am being offered in exchange for categoricals do not, even misleadingly and fatally, invite anyone (except it seems, some philosophers) to look for any "thing" or event in the time series. Whatever is being asserted by, "If it rains, I shall take my umbrella", or "If Hitler had not wanted it, there would have been no war", it will not be found in the inventory of events, in the historical annals of the actual world, nor am I under any impression that I am being invited to look for any such entity. (Only philosophers have gone to the length of searching for or inventing ontological 'referents' of hypothetical propositions.) Hypothetical sentences do, of course, like other empirical expressions, involve the use of words which, to have any meaning, must themselves be capable of occurring in true ostensive sentences which do in some sense 'point'—words like "rain", or "umbrella", or "Hitler", but in themselves hypotheticals do not 'point'; otherwise they would cease to be hypothetical, they would lose their conditional, non-actual-fact-asserting force.

At this point a critic might say (as Professor A. J. Ayer did say to me in discussion), something like this: "You rest your case on the generally felt distinction between what is dispositional and what is non-dispositional in the material world, and say that the latter cannot be described by hypotheticals, as the former can, without doing violence to normal usage. But this is not so. In the first place, many expressions which do not seem dispositional at first, turn out to be so on further analysis: for example, if we say that the table is heavy and six feet long, that seems at first categorical enough, but of course 'heavy' means 'if weighed according to a recognised technique, the instrument will record etc.' and 'six feet long' refers to the possible application of a ruler and so forth: these apparently categorical statements turn out, therefore, to need translation into hypotheticals to make them clear: from which it follows that the categorical form of statement by itself gives no sort of indication of how sentences mean". But this argument establishes less than it appears to do. I should not dream of maintaining that verbal or grammatical form is an infallible guide to logical form, *i.e.* kinds of ways in which sentences mean. Indeed, that is the whole point of exposing the the dispositional character of expressions which *prima facie* appear non-dispositional. But because some or many categoricals are in this sense concealed hypotheticals (*i.e.* their meaning is made clearer, or certain errors are prevented, by the substitution of hypotheticals) because language is flexible and the frontiers shifting and vague, it cannot follow that the distinction does not exist at all, that the frontiers are invisible—for if that were so, such words as "dispositional" and "hypothetical", (there being nothing with which to contrast them) would not signify anything at all. And this is not what phenomenalists or defenders of the theory of logical constructions, if their own words are to mean anything, want to say. At this point the critic may say: "But this is a sheer travesty of my position. Of course I do not wish to blur the useful distinction between hypotheticals and categoricals. What I am asserting is that all descriptive statements can in principle be translated into sense datum language: all material object statements will be transposed into hypothetical statements about sense data, and these are what they are by contrast with the only true ultimate, irreducible categoricals, those describing someone's actual sense experiences: *e.g.* Russell's basic propositions, Carnap's protocol sentences, etc. As for your distinction between dispositional and non-dispositional characteristics of material objects, or between hypothetical and categorical statements as applied to material

objects, the sense datum language is perfectly well able to reproduce it in its own terminology : categorical material object statements will be translated into hypotheticals about sense data ; hypotheticals about material objects will be rendered by hypotheticals about hypotheticals : thus to say that a given table looks brown is to say something about the dispositions of certain observers ; to say that it is fragile is to say something about the dispositions of dispositions of these same observers ; the distinction is one of degree of complexity of hypotheticals ; but the whole pyramid of them only has descriptive force if they are about—if their ultimate subject is—the actual data of actual observers about which all material object sentences, whether categorical or hypothetical, are in the end, hypotheses or theories. For what else is there in the world but what people see and hear and imagine and do and suffer ?” We are there at last : this really is what phenomenism boils down to : that the only irreducibly categorical propositions, by contrast with which alone hypotheticals are what they are, are statements about immediate experience, capable of direct, strong, “ knock-down ” verification. These are basic. All else is theory and speculation about their behaviour and incidence. We have returned to the many-tiered logical constructions, with material objects and perhaps their more obvious causal properties on the floors immediately above the “ basic ” ground floor (or should it be basement ?) and the upper levels occupied by positrons, nerve impulses, super-egos, and possibly vectors and non-Euclidean spaces and numbers too, as well as the *Zeitgeist*, and the British Constitution and the national character. In a sense, this position seems almost too academic in character : if phenomenalists find difficulty, in fact, in producing the sense datum equivalents of even plain categorical material object statements, their claim to produce two or more storeys of such—simple hypotheticals and over these rows of complex ones—hypotheticals about hypotheticals—seems somewhat unreal ; but even if we do not press for cash in the form of basic sentences against phenomenalist cheques (as being unfair and against the spirit of the conventions in use of language) the argument still remains fallacious. For what this view comes to is that material object sentences—including existential ones—are so many general propositions or hypotheses or theories about the behaviour of sense data. And this is precisely what common sense finds so repugnant. For a general proposition or theory may be interpreted purely intensionally—*i.e.* irrespective of whether or not instances of the concepts involved in fact occur ; whereas such a sentence as, “ The table

next door is brown" is existential and as such has extensional import, and asserts that something *is* occurring in a sense in which general or hypothetical propositions proper do not normally assert anything of this sort; if such general propositions are taken extensionally as well as intensionally, *i.e.* if general propositions about sense data are to be understood to assert more than a mere logical or causal nexus between the possible experiences of possible observers, namely, the existence or occurrence of something or other which the nexus connects, then, to perform this task, unsensed sensa or sensibilia must be introduced: and these are rightly as much taboo to phenomenalists as Lockean substances or physical occupants, and a good deal odder in character. The point is that existential material object propositions directly assert that something exists in a sense in which theories or hypotheses do not directly assert this. One can bring out this point most sharply (at the cost of some exaggeration) by asserting baldly that all theories, hypotheses, general and hypothetical propositions, etc., may be true and yet nothing exist at all; for if the protases are unfulfilled, the apodoses have no application; whereas the proposition that some existential material object propositions are true is not compatible with the proposition that nothing exists at all.<sup>1</sup> What this over-simple paradox serves to bring out is that the essence of hypothetical or conditional sentences is to be in a peculiar way non-committal—in the sense in which, let us say, singular (empirical) existential categoricals normally commit the speaker to something which in principle can be directly verified. Now it is notoriously impossible directly to verify unfulfilled conditionals: but all conditionals must entail at least one such unfulfilled conditional, and consequently in this respect cannot be equivalent to statements asserting only what is directly verifiable by an act of observation. Existential categoricals on the other hand, commit us because there is normally an ostensive (pointing) property about existential categorical material object propositions.<sup>2</sup>

<sup>1</sup> This is, of course, not literally true, since theories presuppose the existence of theorists with all that they need by the way of a universe in order to fix the 'grammar' of their words, but this is not part of what the theories themselves assert, nor is it logically entailed by them.

<sup>2</sup> It may be worth adding that such demonstratives as "there is" or "this is" are seldom employed to refer to "sense data"—for that is a term which is rarely of use in ordinary experience, and is more properly applicable to that aspect of things which concern physiologists or oculists or impressionist painters, and is useful precisely because it contrasts that which interests these specialists—purely sensuous qualities—with material objects—things—the furniture of ordinary life.

The same point may be brought out in yet another way. According to the phenomenalist analysis, sentences describing material objects will differ in logical type according to the presence (to my senses) or the absence of the object in question. If it is present, I am said to be acquainted with actual seen data, and my sentence is at least partially analysable into irreducibly categorical ("basic") propositions : if it is absent, it is wholly analysable into hypotheticals. But this is surely not the case : if I say that there is a brown, wooden table in this room, I can, if I like, go on to say that among the propositions which I can assert of it, some are obviously categorical, some plainly hypothetical : some perhaps of neither kind, and then it cannot make a logical difference, *i.e.* a difference of principle, whether the table is before me in the room, or hidden behind a wall : whatever is hypothetically true, *i.e.* dispositional, about the present table (or its visible portion) is doubtless equally hypothetical (dispositional) about the one next door (or its visible portion) : but whatever is categorical about the first is categorical about the other—absent one—too. The actual steps which I am obliged to take in order to *verify* propositions about a given table will, of course, vary with circumstances : if the table is moved out of my ken, or someone blindfolds me, I cannot do what I could have done had this not happened ; but the *meaning* of the sentence which I utter, does not alter with the movements of the table or the condition of my eyes : the meaning of the sentence, "There is a brown table in my study", does not swing forwards and backwards from partially categorical to wholly hypothetical as I move around it, or saw it in half, or walk in and out of my study, or the walls of my study change from opaque to transparent and neither does it wholly consist of a cluster of hypotheticals compatible (if their antecedents are unfulfilled) with the non-existence of any experiences whatever. Perhaps we now see more clearly the confusion from which these odd consequences spring : namely the confounding of the meaning of what we are saying with the varying conditions under which we feel inclined to say it.

## III

At this point, some uneasiness may be felt about the attribution to our language of a capacity to 'point to' objects in absence—as if the transition from pointing to objects directly perceived to this semi-metaphorical sense of pointing, may not be quite legitimate. It is here that the phenomenalist may wish

to play one of his strongest cards, for one of the most tempting advantages which his theory appears to offer is that by substituting logical constructions for inferred entities, he promises to describe the world solely in terms of the so called data of immediate acquaintance. He undertakes, in effect, to describe everything by means of logical or linguistic rules, including rules for the use of conditional participles like "if" and "provided that", and otherwise confine himself solely to what we can directly and literally point to in our everyday experience. And to speak of the ostensive function of a sentence which purports to point towards, direct attention to something—the table—real enough, indeed, but not here and not now, something unobserved, *i.e.* outside the field of direct acquaintance—is this not to go beyond and against the principle of not importing unfamiliar and dubious entities, to contravene the rule of the definability ostensively of all empirical terms? Are we not introducing something not met with face to face, not directly verifiable, and consequently not directly descriptive, perhaps altogether non-empirical? And this may at first unnerve the strict empiricist; but his anxieties will be groundless. For the notion of "not here", "not observed", must in any case be introduced into language seeking to describe the world sooner or later, and how this is accomplished is a psychological rather than an epistemological question. It is one thing to admit that whatever in one's descriptive language is not governed by syntactical rules must be capable of ostensive elucidation: and a very different one to say that I may not refer to anything unless I can establish the meaning of the variables of my language in terms of what I am actually experiencing here and now; if I adopt the latter principle, I become unable to refer to the past or the future or to the experiences of others to explain "here" and "now" and "observed by me", and so on—that way lies the kind of verification theory of meaning which has more than once been shown to lead to an extravagantly solipsist analysis of the meanings of words, ending literally in nonsense. The meaning of such "basic" words as "here", "now", "observed", depend on the existence of an equally "basic" use for "not here", "not now", "not observed", in contrast with which alone the meanings of "here", "now", etc., can be established. There is no need to go on with this line of argument—such comparatively primitive notions as "not now", or "beyond the horizon", cannot be "constructed" without circularity out of sense fields occurring in "specious presents"; but without such notions classification, and therefore language, in the ordinary sense, is demonstrably impossible. Hence, this kind of objection to the possibility

in principle of pointing to objects in absence cannot be considered seriously, for it rests on the assumption (ultimately perhaps traceable to Aristotle's doctrine of actual *v.* potential being) that what is not here does not exist in the same sense of 'exist' as that which is here, which rules out all possibility of descriptive symbolism. For what exists but is not here, exists and is not here, in exactly the same sense of 'exists' as what is—does exist—here. Without this, all words would lose their function of discriminating and classifying.

## IV

There are two final points to be made. (1) Supposing someone were to ask, "But how can we say anything about the table apart from the hypothetical sentences describing what an observer would see if he walked round it, etc. ? Is the table round or oval, dark or light brown, light or heavy ? Surely the sense datum school of philosophy, if it has established nothing else, has made it clear beyond any doubt that these properties in some sense depend on the observer, his physical position, his physiological and psychological condition, etc. Surely the argument from illusion, for example, cannot be dismissed as showing nothing at all because of logical considerations of how different types of sentences are used ? Does the gramophone play tunes in a desert, or to an audience which is stone deaf ? How does the view advanced here differ from the most untenably naive of all forms of naive realism ? " This rejoinder rests on a serious and important confusion which may in part be responsible for the desperate feeling that only phenomenism can somehow, in the end, be true. The theories advanced by physiologists, say about the indispensability of the mechanism of the ear to the hearing of sounds are empirical theories, corroborated by observational and not linguistic tests : and to say, therefore, that the occurrence of a particular kind of hearer is to assert a causal, *i.e.* empirical, and not semantic or logical proposition. I am saying that the event described as the hearing of a sound emitted by a gramophone depends on certain necessary conditions, and amongst these the structure of the hearer's brain or ear occurs in the same sort of way as, let us say, the physical properties of the needle attached to the sound box of the gramophone. But when I analyse propositions about the meaning of sentences, I am certainly not asserting, and need not necessarily be implying, propositions stating causes or conditions of the events which they describe. There may very well in particular cases exist a causal

nexus between the person of the observer and a given material object—what this nexus is, it is the task of the natural sciences to investigate. But this causal nexus is precisely what the phenomenalist,<sup>1</sup> claims *not* to be discussing when he offers a reduction of categorical material object sentences to hypothetical sense datum sentences—if he were, his theory would amount to a queer kind of occasionalism, metaphysical or empirical, according to his view of connections in nature, whereby the observer who figures in the protasis of the phenomenalist hypothetical could destroy a table by averting his gaze as surely as by setting it on fire.<sup>2</sup> When I say that a material object exists or has certain characteristics, I am not, it seems to me, committing myself necessarily to any specific theory about the necessary or sufficient conditions of the existence or character of the object. Hence, the question of when, or for how long, the table next door is coloured brown need not in principle ever affect the answer to the question, “What do I mean when I say, ‘There is a brown table next door’?” This, of course, needs qualification: the meanings of words *are* affected, and often very deeply affected, by our explicit or implicit causal beliefs, and the analysis of what is meant by an expression may very well reveal all kinds of physical or social or psychological beliefs or assumptions prevalent in a given society, a change in which could affect the meaning of words. The degree to which the dispositional characteristics of observers, treated as persons in time and space, enter into the way in which we employ material object words will vary widely: thus, it seems to me reasonably clear that when we say that there is a table next door, we are *not* implying any particular beliefs about the presence or dispositional characteristics of the normal human observer, beyond the fact that if it is a table at all, it must be not wholly invisible, intangible to him, etc.—since otherwise it would not be what we mean by a material object. It seems a little less obvious that I can to-day say that it is coloured brown when not observed, for perhaps by now rudimentary physiological knowledge is sufficiently widespread to have imported into the notion of being coloured certain causal beliefs about the effects in the visual field of changes in our nervous system, etc. It seems very much less clear that I can say that roses smell sweet when no one smells them, or that winds howl when no one hears them, and it seems clearly eccentric to say that heard melodies are sweet, while

<sup>1</sup> For example, Professor A. J. Ayer in *Arist. Soc. Proc.*, 1947.

<sup>2</sup> This is one of the notorious absurdities of which Berkeley is at times guilty, and on which beginners in philosophy are often taught to practise their critical powers.

those unheard are literally sweeter. And all this is doubtless useful in throwing light on our normal usage with regard to such words as, "smell sweet", or "howl", or "sweet melodies", some of which do, while others do not, imply the presence of persons with certain psychological, physiological, etc., attributes as observers. I am merely concerned to show that a quite sufficient number of material object sentences do *not* presuppose such dependence on the existence or behaviour of observers of this kind, that the relation of observers to material objects is more properly to be called an empirical and not a semantic question, however deeply verbal usage and empirical beliefs may be interconnected; and that consequently the view that nothing can in principle be significantly said to occur without explicit and implicit reference to observers is a major fallacy which rests on failure to distinguish between the causal propositions of natural science or common sense and propositions about meaning.

I return to my original point that phenomenism, or at any rate the most prevalent modern form of it, seems to rest on a mistaken analysis of what normal existential material object statements state; they state that things or events existed, or exist, or will exist, or were, are, or will be, characterised by this or that characteristic; and not that something might exist or would exist, or would have existed, the truth (if not the assertion) of which is logically compatible with the non-existence of anything whatever. Even if hypothetical propositions alone describe the conditions without which we should not assert or be justified in asserting the relevant categoricals, yet the meaning of the former is not the same as the meaning of the latter. And this is so, even if we go further and hold, as some do, that the two types of proposition strictly entail one another; since whatever be the sense in which mutual entailment is regarded as tantamount to, or identical with, logical equivalence (as it is by some logicians), it is clearly not the same as the sense of identity of meaning in which two or more descriptive sentences can be said by common sense to mean the same; yet it is this last sense of "meaning the same" as between the analysans and the analysandum, and it alone, that the best known variants of modern phenomenism seek to establish and, if the above thesis is correct, seek in vain.

## II.—BERTRAND RUSSELL ON PROBABILITY

BY HAROLD JEFFREYS

THE analysis of the processes of acquirement of knowledge given in Russell's recent book *Human Knowledge* is the fullest I have seen. He remarks "The apparent publicity of our world is partly delusive and partly inferential—all the raw material of our knowledge consists of mental acts in the lives of separate people" (p. 67). Again, the traveller in France learns that "pain" means "bread", but the French child learns from the habits of its parents that it means bread. "The fact that he succeeds does credit to the infant mind and the veracity of parents" (p. 78). I do not think that psychologists, and particularly psycho-analysts, have paid sufficient attention to this problem, to which I shall return later. On p. 115 Russell states "the primitive reaction to a statement is belief, and understanding without belief involves inhibition of the impulse to belief". This provides an explanation of the common difficulty of understanding "reductio ad absurdum" arguments. In terms of this theory we can state the law of contradiction without speaking of negative facts. "This is not blue" becomes "this is a colour different from blue" and the statement "nothing is both blue and not blue" becomes "A disbelief in the sentence 'the belief that this is blue and the disbelief that this is blue are both true' is always true". Deceptive sensations are dealt with (p. 182); "My visual sensations when I look in a mirror or see double are exactly what I think they are" p. 185: "Only sensations and memories are truly data"—but there is discussion later (p. 412) of the uncertainty of memory. A discussion of solipsism shows (p. 194) that a thoroughgoing solipsist must deny memory—I should admit only what I am remembering now, because I might have come into existence five minutes ago with all the memories that I had then. This argument shows that a deductive solipsist must deny far more than had previously been supposed; and any principles that he might bring in to admit memories as data demand some non-logical inference.

By the way the term "sense-data" does not appear in the index and I have not noticed it in the book, though there is much about sensations. I welcome this disappearance. Classes of actual sensations would not fit together to form a coherent

world, and sense-data as originally defined served no purpose except to make such construction possible. Russell regards the representation of sensations by events in space-time as a construction, and emphasizes the correlation of visual and tactual sensations in this construction. In this he agrees with Karl Pearson's *Grammar of Science*, which he does not mention. He also agrees with Pearson's notion of probability as an epistemological relation, and recognises that inference plays a far larger part than is usually supposed; but he mysteriously attributes this to Keynes instead of to Johnson, Pearson and a series of investigators going back to Bayes and possibly to Leibnitz. Again, he recognises that descriptions are not exact; "Any definition of red that professes to be precise is pretentious and fraudulent (p. 276)" and similarly for recorded measurements. Naturally he rejects the idea that Heisenberg's uncertainty principle has any special philosophical importance. He decides, then, that uncertainty in our knowledge arises in two ways: sensations, and *a fortiori* memories, can be described only within a range of error, and some extra-logical principle is needed to give any ground for inference beyond these data. As I have been maintaining the same for thirty years I welcome Russell's agreement, particularly as he says many things incomparably better than I could ever have hoped to do.

However, Russell's account of the further principles seems to me defective. He begins by discussing "Mathematical Probability", which is defined simply as the ratio of the number of favourable cases to that of all possible cases. He decides that it is not what we need; but I wish that he had condemned the term outright. All probability theories use mathematics, and "mathematical probability" makes no use at all of the notion of a degree of reasonable belief—there is no probability in it. "Combinatory analysis" is available as a name for it. He discusses Reichenbach's limiting frequency theory. So far as this includes the theory of Venn, he makes the same criticisms as Wrinch and I made in 1919; and he finds little use for Reichenbach's additions to allow for alleged unreliability of the data—they either only shift the difficulty or lead to an infinite regress. But when Russell discusses the epistemological theory he shows that he has paid very little attention to any work on it other than that of Keynes. We have all the usual arguments against Laplace's assessment of the prior probability (*a priori* probability is a *nomen confusum* and should be dropped). But he does not state that, once we have adopted an epistemological approach, we are committed to finding a statement of the prior probability

that expresses ignorance between a set of alternatives ; otherwise we have an infinite regress. He agrees that we cannot find it by experiment ; he pronounces Laplace's (really Bayes's) assessment absurd (p. 425) ; but he makes no attempt to put anything specific in its place. Thus he says that uniform prior probability for a density is not equivalent to uniform prior probability for a specific volume ; but one might have thought that, once the problem was put in so direct a way, the answer was obvious—the correct rule is  $d\rho/\rho$ , which is invariant, apart from an irrelevant constant factor, for transformations by powers. Again, Broad found in 1918 that Laplace's rule never leads to a probability approaching certainty that all members of a class of number  $N$  have a property until nearly the whole class has been sampled. Wrinch and I pointed out that this can be avoided if we grant that the prior probability that all members have the property tends to a positive value (not zero) when  $N$  is large. What Laplace's rule says, in fact, is that the prior probability of the general rule is  $1/(N + 1)$ , and it amounts to a denial without evidence that there are any general laws. If we admit that a general law is reasonably likely to be true, we are saying something radically different from Laplace's rule.  $\frac{1}{N}$  for each extreme value (that is, that no member or all members have the property) leads to the interesting result that a generalisation based on one instance has probability  $\frac{1}{2}$ . But I think that the amount of discussion that has been devoted to this type of generalisation is out of all proportion to its practical importance. It is very hard to find true (or even apparently true) instances of it. "All crows are black" is false.<sup>1</sup> I have substituted "all animals with feathers have beaks", but Professor J. B. S. Haldane has pointed out to me that, while this appears to be true of present day animals, it is quite likely to be untrue for Mesozoic ones. The problem is there, of course, but I am inclined to doubt whether it has been properly formulated. The solution should cover, for instance, the identification of a plant by the analytical key in Bentham and Hooker's *Flora*, followed by the verification that the specimen has the other characters stated in the full description.

The position is clearer in relation to quantitative laws. So long as we are concerned with laws of known form, continuous distributions of prior probability are enough to lead to satisfactory

<sup>1</sup> The hooded crow is black and grey ; the magpie is a crow and is black and white. Even the rook and the carrion crow, which are usually black, sometimes have partial or complete albinos. The jay, in the same group, is mainly fawn, and an American jay is blue.

estimates of the parameters taken as constant in the laws. But if we are at liberty to modify a law arbitrarily to any extent we can fit any set of observations exactly, and some of these possibilities would fit any further observation whatever; consequently if there is no limitation on the choice of laws no prediction from observations is possible. Russell quotes me for this argument (p. 388), but leaves the matter in such a way as to suggest that I took no further steps about it. A tentative solution was given by Wrinch and me in 1921 (which various people said was absurd, but they did not answer the arguments leading up to it or suggest any constructive alternative). A different one is given in my *Theory of Probability*, Chapters 5 and 6. This is that where a suggested modification of a law involves an increase in the number of adjustable parameters, half the prior probability is concentrated in the old law; in other words, when a modification is suggested it is as likely to be needed as not. This has been shown to lead to satisfactory significance tests in the standard problems of statistics, though there is much more to be done. The results are of the approximate form

$$\frac{P(q/\theta H)}{P(q'/\theta H)} = \sqrt{(An)} e^{-\frac{1}{2}a^2/s_a^2}$$

Here if the new parameter considered is  $\alpha$ , it is defined so as to be zero on the old law  $q$ , but on the modified law  $q'$  it has to be estimated from the observations;  $H$  is the previous information and  $\theta$  the observational evidence.  $A$  is a constant usually of order 1,  $n$  the number of observations,  $a$  the estimate of  $\alpha$  by the usual statistical methods, and  $s_a$  its standard error. The expression is of order  $\sqrt{n}$  if  $a/s_a$  is less than 1, but very small if  $a/s_a$  is large. Consequently observations support the old law for  $a/s_a < 1$  and the new one if it is large. This choice of the prior probability is what I call the simplicity postulate. Russell says several times that the notion of simplicity is vague; as I use it it is perfectly precise. Of two laws, the simpler is the one that contains fewer parameters left free to be adjusted to fit the observations.

Incidentally this answers Russell's argument that the discovery of Neptune was no confirmation of the law of gravitation, because this might have ceased to hold immediately after the discovery. But the law is stated as for all time. If it changed, the time of the change would be a new adjustable parameter, and a significance test would be needed for its relevance. If we need a special principle for this sort of thing, it is that we can find all the trouble we need in scientific work without looking for it.

In later chapters Russell argues that on the epistemological interpretation of probability it remains possible that inferences made consistently with the theory might nevertheless be habitually wrong; and that some further principle is needed to say that they will be right in an assignable fraction of the cases. Something similar is maintained by M. G. Kendall.<sup>1</sup> Russell suggests a set of five hypotheses (p. 506) such as (p. 508): "It is frequently possible to form a series of events such that, from one or two members of the series, something can be inferred as to all the other members." He rejects the view that causation is merely invariable sequence. I am not convinced about the need for such postulates. They come perilously near denying what I consider to be a fundamental principle, that any suggested law can be confirmed or denied by experience, and consequently there are no *a priori* rules about experience. Also if they were sufficiently precisely stated to be of any practical use they would amount to identifying a prior possibility with a known frequency; but the fundamental role of prior probability is to give a formal expression at the start of an investigation that we do not know in advance what the result will be.

Nevertheless Russell's difficulty is a real one, though I think that the solution, if there is one, is to be found elsewhere. I think that the point is best illustrated by Carnap's recent idea of two kinds of probability. In Carnap's *Logical Syntax of Language* he considers two languages. Language I is an object language, in which we can say "a tomato is red" and "a buttercup is yellow". It could also say "a buttercup is blue" provided it does not contain certain other sentences, such as "nothing is both blue and yellow". (Russell would alter the language to apply directly to sensations, and I would agree with him.) It would not contain the sentence "'A buttercup is blue' is false". That would be a sentence in Language II, which can contain statements about sentences in Language I. Now it would be nice to have a set of rules for an "objective probability" in which a statement "the probability of  $p$  on  $q$  is  $a$ " has a meaning directly about occurrences of  $p$  given  $q$ . Unfortunately, after over a century of effort, nobody has produced such a meaning for it that will hold water. Carnap adopts a long run frequency definition, which would make it impossible to assess a single probability, or to prove any theorem about its rules, at any rate unless we abandon the notion of randomness, which is fundamental in all statistical procedures hitherto. On a definition by enumeration of alternatives we

<sup>1</sup> *Biometrika* 36, 1949, 101-116.

cannot attach any meaning to a statement that a die is biased. However, for the purpose of argument, let us suppose that somebody produces a satisfactory interpretation of objective probability, and that its rules are stated in a language PI. Then most scientific laws (taking account of errors of observation) are statements in PI. To choose between different interpretations of the same data, we need also the epistemological language, which we may call PII. But now it turns out that we can state a consistent set of axioms in PII and by means of them we can prove statements that are in appearance the sort of thing we should like to say in PI. Possibly they mean something different in PII from what they would mean in PI, since they are statements about reasonable degree of belief, which would presumably not occur in PI. When we are talking about their validity, of course, we are using a further language, say PIII. Formally, then, there could be no objection to having a rule in PIII that any statement in PII with a counterpart intelligible in PI is interpreted in PI by that counterpart. There is, of course, the practical objection that PI does not exist; we could interpret in this way "probably  $p$  will happen the next time  $q$  does" as meaning " $p$  will probably happen the next time  $q$  does", but we do not know what the latter sentence means if regarded simply as a statement about  $p$  and  $q$ . I think that it means something, but I do not know what it means. If a satisfactory formulation of PI existed (irrespective of whether we can prove anything in it, because we could adapt the results from PII) there would be no logical difficulty. There is a serious practical difficulty, which is related to my reasons for rejecting hypotheses of this sort outright. Science needs a set of rules of progress, and these have been stated as rules in PII. If the sort of rules stated by Russell were to be true and useful, they must say that after a certain point in investigation it is *a priori* impossible to find out any changes in the laws. But, being addicted to discussing actual observations myself, I see no sign that any of the existing laws are satisfactory in this sense and can feel little hope that any will ever be. Each increase in accuracy of observation seems to lead to the detection of a new lot of systematic variations which then have to be tracked down. To say that there is a limit to this process seems to say that some day there will be nothing for a Newton, Planck, or Rutherford to do. Perhaps this is true, but I think that it will need a greater physicist than any of them to say when it will be true; and I can see no constructive value in saying that it will be true some day without saying when.

The psychological importance of these considerations seems to me extreme. Russell's remarks on the problem of learning a language serve to emphasise their importance. We are still not sure of the answers to the problems of inference, but an infant has to develop some attitude to them before it can learn to speak. Psycho-analysts speak of the Ego as the part of the personality that maintains relations with the external world, and mental disturbances are regarded as failures of the Ego to adjust itself satisfactorily. These are attributed to unconscious emotional attitudes. I do not wish to underestimate the importance of these attitudes, but I do say that even if they were absent the adjustment of the infantile Ego to experience would still be, to all appearance, overwhelmingly difficult. I think that the procedure at the outset is equivalent to attaching probability 1 to every generalisation from one instance; when this is found not to work very well, the reaction is to attach probability  $\frac{1}{2}$  to a generalisation no matter how much evidence there may be. Many adults remain permanently in one state or the other. Such a hypothesis would explain the frequency of fantasy formation and the persistence of fantasies formed in early life. When psycho-analysts say that the logical problems present little difficulty in practice once the emotional ones are cleared up, I am extremely dubious.

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### III.—ON REFERRING

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#### I

WE very commonly use expressions of certain kinds to mention or refer to some individual person or single object or particular event or place or process, in the course of doing what we should normally describe as making a statement about that person, object, place, event, or process. I shall call this way of using expressions the "uniquely referring use". The classes of expressions which are most commonly used in this way are: singular demonstrative pronouns ("this" and "that"); proper names (e.g. "Venice", "Napoleon", "John"); singular personal and impersonal pronouns ("he", "she", "I", "you", "it"); and phrases beginning with the definite article followed by a noun, qualified or unqualified, in the singular (e.g. "the table", "the old man", "the king of France"). Any expression of any of these classes can occur as the subject of what would traditionally be regarded as a singular subject-predicate sentence; and would, so occurring, exemplify the use I wish to discuss.

I do not want to say that expressions belonging to these classes never have any other use than the one I want to discuss. On the contrary, it is obvious that they do. It is obvious that anyone who uttered the sentence, "The whale is a mammal", would be using the expression "the whale" in a way quite different from the way it would be used by anyone who had occasion seriously to utter the sentence, "The whale struck the ship". In the first sentence one is obviously *not* mentioning, and in the second sentence one obviously *is* mentioning, a particular whale. Again if I said, "Napoleon was the greatest French soldier", I should be using the word "Napoleon" to mention a certain individual, but I should not be using the phrase, "the greatest French soldier", to mention an individual, but to say something about an individual I had already mentioned. It would be natural to say that in using this sentence I was talking *about* Napoleon and that what I was *saying* about him was that he was the greatest French soldier. But of course I *could* use the expression, "the greatest French soldier", to mention an individual; for example, by saying: "The greatest French soldier died in exile". So it is obvious that at least some expressions belonging to the classes I mentioned *can* have uses other than the

use I am anxious to discuss. Another thing I do not want to say is that in any given sentence there is never more than one expression used in the way I propose to discuss. On the contrary, it is obvious that there may be more than one. For example, it would be natural to say that, in seriously using the sentence, "The whale struck the ship", I was saying something about both a certain whale and a certain ship, that I was using each of the expressions "the whale" and "the ship" to mention a particular object; or, in other words, that I was using each of these expressions in the uniquely referring way. In general, however, I shall confine my attention to cases where an expression used in this way occurs as the grammatical subject of a sentence.

I think it is true to say that Russell's Theory of Descriptions, which is concerned with the last of the four classes of expressions I mentioned above (*i.e.* with expressions of the form "the so-and-so") is still widely accepted among logicians as giving a correct account of the use of such expressions in ordinary language. I want to show, in the first place, that this theory, so regarded, embodies some fundamental mistakes.

What question or questions about phrases of the form "the so-and-so" was the Theory of Descriptions designed to answer? I think that at least one of the questions may be illustrated as follows. Suppose some one were now to utter the sentence, "The king of France is wise". No one would say that the sentence which had been uttered was meaningless. Everyone would agree that it was significant. But everyone knows that there is not at present a king of France. One of the questions the Theory of Descriptions was designed to answer was the question: how can such a sentence as "The king of France is wise" be significant even when there is nothing which answers to the description it contains, *i.e.*, in this case, nothing which answers to the description "The king of France"? And one of the reasons why Russell thought it important to give a correct answer to this question was that he thought it important to show that another answer which might be given was wrong. The answer that he thought was wrong, and to which he was anxious to supply an alternative, might be exhibited as the conclusion of either of the following two fallacious arguments. Let us call the sentence "The king of France is wise" the sentence S. Then the first argument is as follows:

(1) The phrase, "the king of France", is the subject of the sentence S.

Therefore (2) if S is a significant sentence, S is a sentence *about* the king of France.

But (3) if there in no sense exists a king of France, the sentence is not about anything, and hence not about the king of France.

Therefore (4) since S is significant, there must in some sense (in some world) exist (or subsist) the king of France.

And the second argument is as follows :

- (1) If S is significant, it is either true or false.
- (2) S is true if the king of France is wise and false if the king of France is not wise.
- (3) But the statement that the king of France is wise and the statement that the king of France is not wise are alike true only if there is (in some sense, in some world) something which is the king of France.

Hence (4) since S is significant, there follows the same conclusion as before.

These are fairly obviously bad arguments, and, as we should expect, Russell rejects them. The postulation of a world of strange entities, to which the king of France belongs, offends, he says, against "that feeling for reality which ought to be preserved even in the most abstract studies". The fact that Russell rejects these arguments is, however, less interesting than the extent to which, in rejecting their conclusion, he concedes the more important of their principles. Let me refer to the phrase, "the king of France", as the phrase D. Then I think Russell's reasons for rejecting these two arguments can be summarised as follows. The mistake arises, he says, from thinking that D, which is certainly the *grammatical* subject of S, is also the *logical* subject of S. But D is not the logical subject of S. In fact S, although grammatically it has a singular subject and a predicate, is not logically a subject-predicate sentence at all. The proposition it expresses is a complex kind of *existential* proposition, part of which might be described as a "uniquely existential" proposition. To exhibit the logical form of the proposition, we should re-write the sentence in a logically appropriate grammatical form; in such a way that the deceptive similarity of S to a sentence expressing a subject-predicate proposition would disappear, and we should be safeguarded against arguments such as the bad ones I outlined above. Before recalling the details of Russell's analysis of S, let us notice what his answer, as I have so far given it, seems to imply. His answer seems to imply that in the case of a sentence which is similar to S in that (1) it is grammatically of the subject-predicate form and (2) its grammatical subject does not refer to anything, then the only alternative to its being meaningless is that it should not really (*i.e.* logically) be of the subject-predicate form

at all, but of some quite different form. And this in its turn seems to imply that if there are any sentences which are genuinely of the subject-predicate form, then the very fact of their being significant, having a meaning, guarantees that there *is* something referred to by the logical (and grammatical) subject. Moreover, Russell's answer seems to imply that there are such sentences. For if it is true that one may be misled by the grammatical similarity of *S* to other sentences into thinking that it is logically of the subject-predicate form, then surely there must be other sentences grammatically similar to *S*, which *are* of the subject-predicate form. To show not only that Russell's answer seems to imply these conclusions, but that he accepted at least the first two of them, it is enough to consider what he says about a class of expressions which he calls "logically proper names" and contrasts with expressions, like *D*, which he calls "definite descriptions". Of logically proper names Russell says or implies the following things :

(1) That they and they alone can occur as subjects of sentences which are genuinely of the subject-predicate form ;

(2) that an expression intended to be a logically proper name is *meaningless* unless there is some single object for which it stands : for the *meaning* of such an expression just is the individual object which the expression designates. To be a name at all, therefore, it *must* designate something.

It is easy to see that if anyone believes these two propositions, then the only way for him to save the significance of the sentence *S* is to deny that it is a logically subject-predicate sentence. Generally, we may say that Russell recognises only two ways in which sentences which seem, from their grammatical structure, to be about some particular person or individual object or event, can be significant :

(1) The first is that their grammatical form should be misleading as to their logical form, and that they should be analysable, like *S*, as a special kind of existential sentence ;

(2) The second is that their grammatical subject should be a logically proper name, of which the meaning is the individual thing it designates.

I think that Russell is unquestionably wrong in this, and that sentences which are significant, and which begin with an expression used in the uniquely referring way fall into neither of these two classes. Expressions used in the uniquely referring way are never either logically proper names or descriptions, if what is meant by calling them "descriptions" is that they are to be analysed in

accordance with the model provided by Russell's Theory of Descriptions.

There are no logically proper names and there are no descriptions (in this sense).

Let us now consider the details of Russell's analysis. According to Russell, anyone who asserted S would be asserting that :

- (1) There is a king of France.
- (2) There is not more than one king of France.
- (3) There is nothing which is king of France and is not wise.

It is easy to see both how Russell arrived at this analysis, and how it enables him to answer the question with which we began, *viz.* the question : How can the sentence S be significant when there is no king of France ? The way in which he arrived at the analysis was clearly by asking himself what would be the circumstances in which we would say that anyone who uttered the sentence S had made a true assertion. And it does seem pretty clear, and I have no wish to dispute, that the sentences (1)-(3) above do describe circumstances which are at least *necessary* conditions of anyone making a true assertion by uttering the sentence S. But, as I hope to show, to say this is not at all the same thing as to say that Russell has given a correct account of the use of the sentence S or even that he has given an account which, though incomplete, is correct as far as it goes ; and is certainly not at all the same thing as to say that the model translation provided is a correct model for all (or for any) singular sentences beginning with a phrase of the form "the so-and-so".

It is also easy to see how this analysis enables Russell to answer the question of how the sentence S can be significant, even when there is no king of France. For, if this analysis is correct, anyone who utters the sentence S to-day would be jointly asserting three propositions, one of which (*viz.* that there is a king of France) would be false ; and since the conjunction of three propositions, of which one is false, is itself false, the assertion as a whole would be significant, but false. So neither of the bad arguments for subsistent entities would apply to such an assertion.

## II

As a step towards showing that Russell's solution of his problem is mistaken, and towards providing the correct solution, I want now to draw certain distinctions. For this purpose I shall, for the remainder of this section, refer to an expression which has a uniquely referring use as "an expression" for short ; and to a sentence

beginning with such an expression as "a sentence" for short. The distinctions I shall draw are rather rough and ready, and, no doubt, difficult cases could be produced which would call for their refinement. But I think they will serve my purpose. The distinctions are between :

- (A1) a sentence,
- (A2) a use of a sentence,
- (A3) an utterance of a sentence,

and, correspondingly, between :

- (B1) an expression,
- (B2) a use of an expression,
- (B3) an utterance of an expression.

Consider again the sentence, "The king of France is wise". It is easy to imagine that this sentence was uttered at various times from, say, the beginning of the seventeenth century onwards, during the reigns of each successive French monarch ; and easy to imagine that it was also uttered during the subsequent periods in which France was not a monarchy. Notice that it was natural for me to speak of "the sentence" or "this sentence" being uttered at various times during this period ; or, in other words, that it would be natural and correct to speak of *one and the same* sentence being uttered on all these various occasions. It is in the sense in which it would be correct to speak of one and the same sentence being uttered on all these various occasions that I want to use the expression (A1) "a sentence". There are, however, obvious differences between different *occasions of the use* of this sentence. For instance, if one man uttered it in the reign of Louis XIV and another man uttered it in the reign of Louis XV, it would be natural to say (to assume) that they were respectively talking about different people ; and it might be held that the first man, in using the sentence, made a true assertion, while the second man, in using the same sentence, made a false assertion. If on the other hand two different men simultaneously uttered the sentence (*e.g.* if one wrote it and the other spoke it) during the reign of Louis XIV, it would be natural to say (assume) that they were both talking about the same person, and, in that case, in using the sentence, they *must* either both have made a true assertion or both have made a false assertion. And this illustrates what I mean by *a use* of a sentence. The two men who uttered the sentence, one in the reign of Louis XV and one in the reign of Louis XIV, each made a different use of the same sentence ; whereas the two men who uttered the sentence simultaneously in the reign of Louis XIV,

made the same use<sup>1</sup> of the same sentence. Obviously in the case of this sentence, and equally obviously in the case of many others, we cannot talk of *the sentence* being true or false, but only of its being used to make a true or false assertion, or (if this is preferred) to express a true or a false proposition. And equally obviously we cannot talk of *the sentence* being *about* a particular person, for the same sentence may be used at different times to talk about quite different particular persons, but only of *a use* of the sentence to talk about a particular person. Finally it will make sufficiently clear what I mean by an utterance of a sentence if I say that the two men who simultaneously uttered the sentence in the reign of Louis XIV made two different utterances of the same sentence, though they made the same *use* of the sentence.

If we now consider not the whole sentence, "The king of France is wise", but that part of it which is the expression, "the king of France", it is obvious that we can make analogous, though not identical distinctions between (1) the expression, (2) a use of the expression and (3) an utterance of the expression. The distinctions will not be identical; we obviously cannot correctly talk of the expression "the king of France" being used to express a true or false proposition, since in general only sentences can be used truly or falsely; and similarly it is only by using a sentence and not by using an expression alone, that you can talk about a particular person. Instead, we shall say in this case that you *use* the expression to *mention* or *refer to* a particular person in the course of using the sentence to talk about him. But obviously in this case, and a great many others, the *expression* (B1) cannot be said to mention, or refer to, anything, any more than the *sentence* can be said to be true or false. The same expression can have different mentioning-uses, as the same sentence can be used to make statements with different truth-values. "Mentioning", or "referring", is not something an expression does; it is something that some one can use an expression to do. Mentioning, or referring to, something is a characteristic of *a use* of an expression, just as "being about" something, and truth-or-falsity, are characteristics of *a use* of a sentence.

A very different example may help to make these distinctions clearer. Consider another case of an expression which has a

<sup>1</sup> This usage of 'use' is, of course, different from (a) the current usage in which 'use' (of a particular word, phrase, sentence) = (roughly) 'rules for using' = (roughly) 'meaning'; and from (b) my own usage in the phrase "uniquely referring use of expressions" in which 'use' = (roughly) 'way of using'.

uniquely referring use, *viz.* the expression "I"; and consider the sentence, "I am hot". Countless people may use this same sentence; but it is logically impossible for two different people to make *the same use* of this sentence: or, if this is preferred, to use it to express the same proposition. The expression "I" may correctly be used by (and only by) any one of innumerable people to refer to himself. To say this is to say something about the expression "I": it is, in a sense, to give its meaning. This is the sort of thing that can be said about *expressions*. But it makes no sense to say of the expression "I" that it refers to a particular person. This is the sort of thing that can be said only of a particular use of the expression.

Let me use "type" as an abbreviation for "sentence or expression". Then I am not saying that there are sentences and expression (types), *and* uses of them, *and* utterances of them, as there are ships *and* shoes *and* sealing-wax. I am saying that we cannot say *the same things* about types, uses of types, and utterances of types. And the fact is that we do talk about types; and that confusion is apt to result from the failure to notice the differences between what we can say about these and what we can say only about the *uses* of types. We are apt to fancy we are talking about sentences and expressions when we are talking about the uses of sentences and expressions.

This is what Russell does. Generally, as against Russell, I shall say this. Meaning (in at least one important sense) is a function of the sentence or expression; mentioning and referring and truth or falsity, are functions of the use of the sentence or expression. To give the meaning of an expression (in the sense in which I am using the word) is to give *general directions* for its use to refer to or mention particular objects or persons; to give the meaning of a sentence is to give *general directions* for its use in making true or false assertions. It is not to talk about any particular occasion of the use of the sentence or expression. The meaning of an expression cannot be identified with the object it is used, on a particular occasion, to refer to. The meaning of a sentence cannot be identified with the assertion it is used, on a particular occasion, to make. For to talk about the meaning of an expression or sentence is not to talk about its use on a particular occasion, but about the rules, habits, conventions governing its correct use, on all occasions, to refer or to assert. So the question of whether a sentence or expression is *significant or not* has nothing whatever to do with the question of whether the sentence, *uttered on a particular occasion*, is, on that occasion, being used to make a true-or-false assertion or not, or of whether

the expression is, on that occasion, being used to refer to, or mention, anything at all.

The source of Russell's mistake was that he thought that referring or mentioning, if it occurred at all, must be meaning. He did not distinguish B1 from B2 ; he confused expressions with their use in a particular context ; and so confused meaning with mentioning, with referring. If I talk about my handkerchief, I can, perhaps, produce the object I am referring to out of my pocket. I can't produce the meaning of the expression, "my handkerchief", out of my pocket. Because Russell confused meaning with mentioning, he thought that if there were any expressions having a uniquely referring use, which were what they seemed (*i.e.* logical subjects) and not something else in disguise, their meaning must be the particular object which they were used to refer to. Hence the troublesome mythology of the logically proper name. But if some one asks me the meaning of the expression "this"—once Russell's favourite candidate for this status—I do not hand him the object I have just used the expression to refer to, adding at the same time that the meaning of the word changes every time it is used. Nor do I hand him all the objects it ever has been, or might be, used to refer to. I explain and illustrate the conventions governing the use of the expression. This is giving the meaning of the expression. It is quite different from giving (in any sense of giving) the object to which it refers ; for the expression itself does not refer to anything ; though it can be used, on different occasions, to refer to innumerable things. Now as a matter of fact there is, in English, a sense of the word "mean" in which this word does approximate to "indicate, mention or refer to" ; *e.g.* when somebody (unpleasantly) says, "I mean you" ; or when I point and say, "That's the one I mean". But the one I meant is quite different from the meaning of the expression I used to talk of it. In this special sense of "mean", it is people who mean, not expressions. People use expressions to refer to particular things. But the meaning of an expression is not the set of things or the single thing it may correctly be used to refer to : the meaning is the set of rules, habits, conventions for its use in referring.

It is the same with sentences : even more obviously so. Every one knows that the sentence, "The table is covered with books", is significant, and every one knows what it means. But if I ask, "What object is that sentence about ?" I am asking an absurd question—a question which cannot be asked about the sentence, but only about some use of the sentence : and in this case the sentence hasn't been used, it has only been taken as an example.

In knowing what it means, you are knowing how it could correctly be used to talk about things: so knowing the meaning hasn't anything to do with knowing about any particular use of the sentence to talk about anything. Similarly, if I ask: "Is the sentence true or false?" I am asking an absurd question, which becomes no less absurd if I add, "It must be one or the other since it's significant". The question is absurd, because the sentence is neither true nor false any more than it's *about* some object. Of course the fact that it's significant is the same as the fact that it *can* correctly be used to talk about something and that, in so using it, some one will be making a true or false assertion. And I will add that it will be used to make a true or false assertion *only* if the person using it *is* talking about something. If, when he utters it, he is not talking about anything, then his use is not a genuine one, but a spurious or pseudo-use: he is not making either a true or a false assertion, though he may think he is. And this points the way to the correct answer to the puzzle to which the Theory of Descriptions gives a fatally incorrect answer. The important point is that the question of whether the sentence is significant or not is quite independent of the question that can be raised about a particular use of it, *viz.* the question whether it is a genuine or a spurious use, whether it is being used to talk about something, or in make-believe, or as an example in philosophy. The question whether the sentence is significant or not is the question whether there exist such language habits, conventions or rules that the sentence logically could be used to talk about something; and is hence quite independent of the question whether it is being so used on a particular occasion.

### III

Consider again the sentence, "The king of France is wise", and the true and false things Russell says about it.

There are at least two true things which Russell would say about the sentence:

(1) The first is that it is significant; that if anyone were now to utter it, he would be uttering a significant sentence.

(2) The second is that anyone now uttering the sentence would be making a true assertion only if there in fact at present existed one and only one king of France, and if he were wise.

What are the false things which Russell would say about the sentence? They are:

(1) That anyone now uttering it would be making a true assertion or a false assertion;

(2) That part of what he would be asserting would be that there at present existed one and only one king of France.

I have already given some reasons for thinking that these two statements are incorrect. Now suppose some one were in fact to say to you with a perfectly serious air : "The king of France is wise". Would you say, "That's untrue"? I think it's quite certain that you wouldn't. But suppose he went on to *ask* you whether you thought that what he had just said was true, or was false; whether you agreed or disagreed with what he had just said. I think you would be inclined, with some hesitation, to say that you didn't do either; that the question of whether his statement was true or false simply *didn't arise*, because there was no such person as the king of France.<sup>1</sup> You might, if he were obviously serious (had a dazed astray-in-the-centuries look), say something like: "I'm afraid you must be under a misapprehension. France is not a monarchy. There is no king of France." And this brings out the point that if a man seriously uttered the sentence, his uttering it would in some sense be *evidence* that he *believed* that there was a king of France. It would not be evidence for his believing this simply in the way in which a man's reaching for his raincoat is evidence for his believing that it is raining. But nor would it be evidence for his believing this in the way in which a man's saying, "It's raining" is evidence for his believing that it is raining. We might put it as follows. To say, "The king of France is wise" is, in some sense of "imply", to *imply* that there is a king of France. But this is a very special and odd sense of "imply". "Implies" in this sense is certainly not equivalent to "entails" (or "logically implies"). And this comes out from the fact that when, in response to his statement, we say (as we should) "There is no king of France", we should certainly *not* say we were *contradicting* the statement that the king of France is wise. We are certainly not saying that it's false. We are, rather, giving a reason for saying that the question of whether it's true or false simply doesn't arise.

And this is where the distinction I drew earlier can help us. The sentence, "The king of France is wise", is certainly significant; but this does not mean that any particular use of it is true or false. We use it truly or falsely when we use it to talk about some one; when, in using the expression, "The king of France", we are in fact mentioning some one. The fact that the sentence and the expression, respectively, are significant just is the fact that the sentence *could* be used, in certain circumstances, to

<sup>1</sup> Since this article was written, there has appeared a clear statement of this point by Mr Geach in *Analysis* Vol. 10, No. 4, March, 1950.

say something true or false, that the expression *could* be used, in certain circumstances to mention a particular person; and to know their meaning is to know what sort of circumstances these are. So when we utter the sentence without in fact mentioning anybody by the use of the phrase, "The king of France", the sentence doesn't cease to be significant: we simply *fail* to say anything true or false because we simply fail to mention anybody by this particular use of that perfectly significant phrase. It is, if you like, a spurious use of the sentence, and a spurious use of the expression; though we may (or may not) mistakenly think it a genuine use.

And such spurious uses are very familiar. Sophisticated romancing, sophisticated fiction,<sup>1</sup> depend upon them. If I began, "The king of France is wise", and went on, "and he lives in a golden castle and has a hundred wives", and so on, a hearer would understand me perfectly well, without supposing *either* that I was talking about a particular person, *or* that I was making a false statement to the effect that there existed such a person as my words described. (It is worth adding that where the use of sentences and expressions is overtly fictional, the sense of the word "about" may change. As Moore said, it is perfectly natural and correct to say that some of the statements in *Pickwick Papers* are *about* Mr. Pickwick. But where the use of sentences and expressions is not overtly fictional, this use of "about" seems less correct; *i.e.* it would not *in general* be correct to say that a statement was about Mr. X or the so-and-so, unless there were such a person or thing. So it is where the romancing is in danger of being taken seriously that we might answer the question, "Who is he talking about?" with "He's not talking about anybody"; but, in saying this, we are not saying that what he is saying is either false or nonsense.)

Overtly fictional uses apart, however, I said just now that to use such an expression as "The king of France" at the beginning of a sentence was, in some sense of "imply", to imply that there was a king of France. When a man uses such an expression, he does not *assert*, nor does what he says *entail*, a uniquely existential proposition. But one of the conventional functions of the definite article is to act as a *signal* that a unique reference is being made—a signal, not a disguised assertion. When we begin a sentence with "the such-and-such" the use of "the" shows, but does not state, that we are, or intend to be, referring to one particular individual of the species "such-and-such". *Which* particular individual is a matter to be determined from context, time, place

<sup>1</sup> The unsophisticated kind begins: "Once upon time there was . . .".

and any other features of the situation of utterance. Now, whenever a man uses any expression, the presumption is that he thinks he is using it correctly: so when he uses the expression, "the such-and-such", in a uniquely referring way, the presumption is that he thinks both that there is *some* individual of that species, and that the context of use will sufficiently determine which one he has in mind. To use the word "the" in this way is then to imply (in the relevant sense of "imply") that the existential conditions described by Russell are fulfilled. But to use "the" in this way is not to *state* that those conditions are fulfilled. If I begin a sentence with an expression of the form, "the so-and-so", and then am prevented from saying more, I have made no statement of any kind; but I may have succeeded in mentioning some one or something.

The uniquely existential assertion supposed by Russell to be part of any assertion in which a uniquely referring use is made of an expression of the form "the so-and-so" is, he observes, a compound of two assertions. To say that there is a  $\phi$  is to say something compatible with there being several  $\phi$ s; to say there is not more than one  $\phi$  is to say something compatible with there being none. To say there is one  $\phi$  and one only is to compound these two assertions. I have so far been concerned mostly with the alleged assertion of existence and less with the alleged assertion of uniqueness. An example which throws the emphasis on to the latter will serve to bring out more clearly the sense of "implied" in which a uniquely existential assertion is implied, but not entailed, by the use of expressions in the uniquely referring way. Consider the sentence, "The table is covered with books". It is quite certain that in any normal use of this sentence, the expression "the table" would be used to make a unique reference, *i.e.* to refer to some one table. It is a quite strict use of the definite article, in the sense in which Russell talks on p. 30 of *Principia Mathematica*, of using the article "*strictly*, so as to imply uniqueness". On the same page Russell says that a phrase of the form "the so-and-so", used strictly, "will only have an application in the event of there being one so-and-so and no more". Now it is obviously quite false that the phrase "the table" in the sentence "the table is covered with books", used normally, will "only have an application in the event of there being one table and no more". It is indeed tautologically true that, in such a use, the phrase will have an application only in the event of there being one table and no more *which is being referred to*, and that it will be understood to have an application only in the event of there being one table and no more *which it is understood as being used to refer to*.

To use the sentence is not to assert, but it is (in the special sense discussed) to imply, that there is only one thing which is *both* of the kind specified (*i.e.* a table) *and is being referred to* by the speaker. It is obviously not to assert this. To refer is not to say you are referring. To say there is *some table or other* to which you are referring is not the same as referring to a particular table. We should have no use for such phrases as "the individual I referred to" unless there were something which counted as referring. (It would make no sense to say you had pointed if there were nothing which counted as pointing.) So once more I draw the conclusion that referring to or mentioning a particular thing cannot be dissolved into any kind of assertion. To refer is not to assert, though you refer in order to go on to assert.

Let me now take an example of the uniquely referring use of an expression not of the form, "the so-and-so". Suppose I advance my hands, cautiously cupped, towards someone, saying, as I do so, "This is a fine red one". He, looking into my hands and seeing nothing there, may say: "What is? What are you talking about?" Or perhaps, "But there's nothing in your hands". Of course it would be absurd to say that in saying "But you've got nothing in your hands", he was *denying* or *contradicting* what I said. So "this" is not a disguised description in Russell's sense. Nor is it a logically proper name. For one must know what the sentence means in order to react in that way to the utterance of it. It is precisely because the significance of the word "this" is independent of any particular reference it may be used to make, though not independent of the way it may be used to refer, that I can, as in this example, use it to *pretend* to be referring to something.

The general moral of all this is that communication is much less a matter of explicit or disguised assertion than logicians used to suppose. The particular application of this general moral in which I am interested is its application to the case of making a unique reference. It is a part of the significance of expressions of the kind I am discussing that they can be used, in an immense variety of contexts, to make unique references. It is no part of their significance to assert that they are being so used or that the conditions of their being so used are fulfilled. So the wholly important distinction we are required to draw is between :

- (1) using an expression to make a unique reference ; and
- (2) asserting that there is one and only one individual which has certain characteristics (*e.g.* is of a certain kind, or stands in a certain relation to the speaker, or both).

This is, in other words, the distinction between

- (1) sentences containing an expression used to indicate or mention or refer to a particular person or thing ; and
- (2) uniquely existential sentences.

What Russell does is progressively to assimilate more and more sentences of class (1) to sentences of class (2), and consequently to involve himself in insuperable difficulties about logical subjects, and about values for individual variables generally : difficulties which have led him finally to the logically disastrous theory of names developed in the *Enquiry* and in *Human Knowledge*. That view of the meaning of logical-subject-expressions which provides the whole incentive to the Theory of Descriptions at the same time precludes the possibility of Russell's ever finding any satisfactory substitutes for those expressions which, beginning with substantival phrases, he progressively degrades from the status of logical subjects.<sup>1</sup> It is not simply, as is sometimes said, the fascination of the relation between a name and its bearer, that is the root of the trouble. Not even names come up to the impossible standard set. It is rather the combination of two more radical misconceptions : first, the failure to grasp the importance of the distinction (section II above) between what may be said of an expression and what may be said of a particular use of it ; second, a failure to recognise the uniquely referring use of expressions for the harmless, necessary thing it is, distinct from, but complementary to, the predicative or ascriptive use of expressions. The expressions which can in fact occur as singular logical subjects are expressions of the class I listed at the outset (demonstratives, substantival phrases, proper names, pronouns) : to say this is to say that these expressions, together with context (in the widest sense) are what one uses to make unique references. The point of the conventions governing the uses of such expressions is, along with the situation of utterance, to secure uniqueness of reference. But to do this, enough is enough. We do not, and we cannot, while referring, attain the point of complete explicitness at which the referring function is no longer performed. The actual unique reference made, if any, is a matter of the particular use in the particular context ; the significance of the expression used is the set of rules or conventions which permit such references to be made. Hence we can, using significant expressions, pretend to refer, in make-believe or in fiction, or mistakenly think we are referring when we are not referring to anything.

This shows the need for distinguishing two kinds (among many

<sup>1</sup> And this in spite of the danger-signal of that phrase, "misleading grammatical form".

others) of linguistic conventions or rules: rules for referring, and rules for attributing and ascribing; and for an investigation of the former. If we recognise this distinction of use for what it is, we are on the way to solving a number of ancient logical and metaphysical puzzles.

My last two sections are concerned, but only in the barest outline, with these questions.

#### IV

One of the main purposes for which we use language is the purpose of stating facts about things and persons and events. If we want to fulfil this purpose, we must have some way of forestalling the question, "What (who, which one) are you talking about?" as well as the question, "What are you saying about it (him, her)?" The task of forestalling the first question is the referring (or identifying) task. The task of forestalling the second is the attributive (or descriptive or classificatory or ascriptive) task. In the conventional English sentence which is used to state, or to claim to state, a fact about an individual thing or person or event, the performance of these two tasks can be roughly and approximately assigned to separable expressions.<sup>1</sup> And in such a sentence, this assigning of expressions to their separate roles corresponds to the conventional grammatical classification of subject and predicate. There is nothing sacrosanct about the employment of separable expressions for these two tasks. Other methods could be, and are, employed. There is, for instance, the method of uttering a single word or attributive phrase in the conspicuous presence of the object referred to; or that analogous method exemplified by, *e.g.* the painting of the words "unsafe for lorries" on a bridge, or the tying of a label reading "first prize" on a vegetable marrow. Or one can imagine an elaborate game in which one never used an expression in the uniquely referring way at all, but uttered only uniquely existential sentences, trying to enable the hearer to identify what was being talked of by means of an accumulation of relative clauses. (This description of the purposes of the game shows in what sense it would be a game: this is not the normal use we make of existential sentences.) Two points require emphasis. The first is that the necessity of performing these two tasks in order to state particular facts requires no transcendental explanation: to call attention to it is partly to elucidate the meaning of the phrase, "stating a fact". The second is that even this elucidation is made in terms derivative from the grammar

<sup>1</sup> I neglect relational sentences; for these require, not a modification in the principle of what I say, but a complication of the detail.

of the conventional singular sentence ; that even the overtly functional, linguistic distinction between the identifying and attributive roles that words may play in language is prompted by the fact that ordinary speech offers us separable expressions to which the different functions may be plausibly and approximately assigned. And this functional distinction has cast long philosophical shadows. The distinctions between particular and universal, between substance and quality, are such pseudo-material shadows, cast by the grammar of the conventional sentence, in which separable expressions play distinguishable roles.

To use a separate expression to perform the first of these tasks is to use an expression in the uniquely referring way. I want now to say something in general about the conventions of use for expressions used in this way, and to contrast them with conventions of ascriptive use. I then proceed to the brief illustration of these general remarks and to some further applications of them.

What in general is required for making a unique reference is, obviously, some device, or devices, for showing both *that* a unique reference is intended and *what* unique reference it is ; some device requiring and enabling the hearer or reader to identify what is being talked about. In securing this result, the context of utterance is of an importance which it is almost impossible to exaggerate ; and by "context" I mean, at least, the time, the place, the situation, the identity of the speaker, the subjects which form the immediate focus of interest, and the personal histories of both the speaker and those he is addressing. Besides context, there is, of course, convention ;—linguistic convention. But, except in the case of genuine proper names, of which I shall have more to say later, the fulfilment of more or less precisely stateable contextual conditions is *conventionally* (or, in a wide sense of the word, *logically*) required for the correct referring use of expressions in a sense in which this is not true of correct ascriptive uses. The requirement for the correct application of an expression in its ascriptive use to a certain thing is simply that the thing should be of a certain kind, have certain characteristics. The requirement for the correct application of an expression in its referring use to a certain thing is something over and above any requirement derived from such ascriptive meaning as the expression may have ; it is, namely, the requirement that the thing should be in a certain relation to the speaker and to the context of utterance. Let me call this the contextual requirement. Thus, for example, in the limiting case of the word "I" the contextual requirement is that the thing should be identical with the speaker ; but in the case of

most expressions which have a referring use this requirement cannot be so precisely specified. A further, and perfectly general, difference between conventions for referring and conventions for describing is one we have already encountered, *viz.* that the fulfilment of the conditions for a correct ascriptive use of an expression is a part of what is stated by such a use; but the fulfilment of the conditions for a correct referring use of an expression is never part of what is stated, though it is (in the relevant sense of "implied") implied by such a use.

Conventions for referring have been neglected or misinterpreted by logicians. The reasons for this neglect are not hard to see, though they are hard to state briefly. Two of them are, roughly: (1) the preoccupation of most logicians with definitions; (2) the preoccupation of some logicians with formal systems. (1) A definition, in the most familiar sense, is a specification of the conditions of the correct ascriptive or classificatory use of an expression. Definitions take no account of contextual requirements. So that in so far as the search for the meaning or the search for the analysis of an expression is conceived as the search for a definition, the neglect or misinterpretation of conventions other than ascriptive is inevitable. Perhaps it would be better to say (for I do not wish to legislate about "meaning" or "analysis") that logicians have failed to notice that problems of use are wider than problems of analysis and meaning. (2) The influence of the preoccupation with mathematics and formal logic is most clearly seen (to take no more recent examples) in the cases of Leibniz and Russell. The constructor of calculuses, not concerned or required to make factual statements, approaches applied logic with a prejudice. It is natural that he should assume that the types of convention with whose adequacy in one field he is familiar should be really adequate, if only one could see how, in a quite different field—that of statements of fact. Thus we have Leibniz striving desperately to make the uniqueness of unique references a matter of logic in the narrow sense, and Russell striving desperately to do the same thing, in a different way, both for the implication of uniqueness and for that of existence.

It should be clear that the distinction I am trying to draw is primarily one between different rôles or parts that expressions may play in language, and not primarily one between different groups of expressions; for some expressions may appear in either rôle. Some of the kinds of words I shall speak of have predominantly, if not exclusively, a referring rôle. This is most obviously true of pronouns and ordinary proper names. Some can occur

as wholes or parts of expressions which have a predominantly referring use, and as wholes or parts of expressions which have a predominantly ascriptive or classificatory use. The obvious cases are common nouns ; or common nouns preceded by adjectives, including participial adjectives ; or, less obviously, adjectives or participial adjectives alone. Expressions capable of having a referring use also differ from one another in at least the three following, not mutually independent, ways :

- (1) They differ in the extent to which the reference they are used to make is dependent on the context of their utterance. Words like "I" and "it" stand at one end of this scale—the end of maximum dependence—and phrases like "the author of Waverley" and "the eighteenth king of France" at the other.
- (2) They differ in the degree of "descriptive meaning" they possess : by "descriptive meaning" I intend "conventional limitation, in application, to things of a certain general kind, or possessing certain general characteristics". At one end of this scale stand the proper names we most commonly use in ordinary discourse ; men, dogs and motor-bicycles may be called "Horace". The pure name has no descriptive meaning (except such as it may acquire *as a result of* some one of its uses as a name). A word like "he" has minimal descriptive meaning, but has some. Substantival phrases like "the round table" have the maximum descriptive meaning. An interesting intermediate position is occupied by 'impure' proper names like "The Round Table"—substantival phrases which have grown capital letters.
- (3) Finally, they may be divided into the following two classes :  
 (i) those of which the correct referring use is regulated by some *general* referring-cum-ascriptive conventions. To this class belong both pronouns, which have the least descriptive meaning, and substantival phrases which have the most ;  
 (ii) those of which the correct referring use is regulated by no general conventions, either of the contextual or the ascriptive kind, but by conventions which are *ad hoc* for each particular use (though not for each particular utterance). Roughly speaking, the most familiar kind of proper names belong to this class. Ignorance of a man's name is not ignorance of the language. This is why we do not speak of the meaning of proper names. (But it won't do to say they are meaningless.) Again an intermediate position

is occupied by such phrases as "The Old Pretender" Only an old pretender may be so referred to ; but to know which old pretender is not to know a general, but an *ad hoc*, convention.

In the case of phrases of the form "the so-and-so" used referringly, the use of "the" together with the position of the phrase in the sentence (*i.e.* at the beginning, or following a transitive verb or preposition) acts as a signal *that* a unique reference is being made ; and the following noun, or noun and adjective, together with the context of utterance, shows *what* unique reference is being made. In general the functional difference between common nouns and adjectives is that the former are naturally and commonly used referringly, while the latter are not commonly, or so naturally, used in this way, except as qualifying nouns ; though they can be and are, so used alone. And of course this functional difference is not independent of the descriptive force peculiar to each word. In general we should expect the descriptive force of nouns to be such that they are more efficient tools for the job of showing what unique reference is intended when such a reference is signalled ; and we should also expect the descriptive force of the words we naturally and commonly use to make unique reference to mirror our interest in the salient, relatively permanent and behavioural characteristics of things. These two expectations are not independent of one another ; and, if we look at the differences between the commoner sort of common nouns and the commoner sort of adjectives, we find them both fulfilled. These are differences of the kind that Locke quaintly reports, when he speaks of our ideas of substances being *collections* of simple ideas ; when he says that "powers make up a great part of our ideas of substances" ; and when he goes on to contrast the identity of real and nominal essence in the case of simple ideas with their lack of identity and the shiftingness of the nominal essence in the case of substances. "Substance" itself is the troublesome tribute Locke pays to his dim awareness of the difference in predominant linguistic function that lingered even when the noun had been expanded into a more or less indefinite string of adjectives. Russell repeats Locke's mistake with a difference when, admitting the inference from syntax to reality to the extent of feeling that he can get rid of this metaphysical unknown only if he can purify language of the referring function altogether, he draws up his programme for "abolishing particulars" ; a programme, in fact, for abolishing the distinction of logical use which I am here at pains to emphasise.

The contextual requirement for the referring use of pronouns may be stated with the greatest precision in some cases (e.g. "I" and "you") and only with the greatest vagueness in others ("it" and "this"). I propose to say nothing further about pronouns, except to point to an additional symptom of the failure to recognise the uniquely referring use for what it is; the fact, namely, that certain logicians have actually sought to elucidate the nature of a variable by offering such *sentences* as "he is sick", "it is green", as examples of something in ordinary speech like a *sentential function*. Now of course it is true that the word "he" may be used on different occasions to refer to different people or different animals: so may the word "John" and the phrase "the cat". What deters such logicians from treating these two expressions as quasi-variables is, in the first case, the lingering superstition that a name is logically tied to a single individual, and, in the second case, the descriptive meaning of the word "cat". But "he", which has a wide range of applications and minimal descriptive force, only acquires a use as a referring word. It is this fact, together with the failure to accord to expressions used referringly, the place in logic which belongs to them (the place held open for the mythical logically proper name), that accounts for the misleading attempt to elucidate the nature of the variable by reference to such words as "he", "she", "it".

Of ordinary proper names it is sometimes said that they are essentially words each of which is used to refer to just one individual. This is obviously false. Many ordinary personal names—names *par excellence*—are correctly used to refer to numbers of people. An ordinary personal name, is, roughly, a word, used referringly, of which the use is *not* dictated by any descriptive meaning the word may have, and is *not* prescribed by any such general rule for use as a referring expression (or a part of a referring expression) as we find in the case of such words as "I", "this" and "the", but is governed by *ad hoc* conventions for each particular set of applications of the word to a given person. The important point is that the correctness of such applications does not follow from any *general* rule or convention for the use of the word as such. (The limit of absurdity and obvious circularity is reached in the attempt to treat names as disguised description in Russell's sense; for what is in the special sense implied, but not entailed, by my now referring to some one by name is simply the existence of some one, *now being referred to*, who is *conventionally referred to* by that name.) Even this feature of names, however, is only a symptom of the purpose for which they are employed. At present our choice of names is

partly arbitrary, partly dependent on legal and social observances. It would be perfectly possible to have a thorough-going *system* of names, based *e.g.* on dates of birth, or on a minute classification of physiological and anatomical differences. But the success of any such system would depend entirely on the convenience of the resulting name-allotments for the purpose of making unique references; and this would depend on the multiplicity of the classifications used and the degree to which they cut haphazard across normal social groupings. Given a sufficient degree of both, the selectivity supplied by context would do the rest; just as is the case with our present naming habits. Had we such a system, we could use name-words descriptively (as we do at present, to a limited extent and in a different way, with some famous names) as well as referringly. But it is by criteria derived from consideration of the requirements of the referring task that we should assess the adequacy of any system of naming. From the naming point of view, no kind of classification would be better or worse than any other simply because of the kind of classification—natal or anatomical—that it was.

I have already mentioned the class of quasi-names, of substantival phrases which grow capital letters, and of which such phrases as “the Glorious Revolution”, “the Great War”, “the Annunciation”, “the Round Table” are examples. While the descriptive meaning of the words which follow the definite article is still relevant to their referring role, the capital letters are a sign of that extra-logical selectivity in their referring use, which is characteristic of pure names. Such phrases are found in print or in writing when one member of some class of events or things is of quite outstanding interest in a certain society. These phrases are embryonic names. A phrase may, for obvious reasons, pass into, and out of, this class (*e.g.* “the Great War”).

## V

I want to conclude by considering, all too briefly, three further problems about referring uses.

(a) *Indefinite references.* Not all referring uses of singular expressions forestall the question “What (who, which one) are you talking about?” There are some which either invite this question, or disclaim the intention or ability to answer it. Examples are such sentence-beginnings as “A man told me that . . .”, “Some one told me that. . . .” The orthodox (Russellian) doctrine is that such sentences are existential, but not uniquely existential. This seems wrong in several ways. It is ludicrous to

suggest that part of what is asserted is that the class of men or persons is not empty. Certainly this is *implied* in the by now familiar sense of implication ; but the implication is also as much an implication of the *uniqueness* of the particular object of reference as when I begin a sentence with such a phrase as "the table". The difference between the use of the definite and indefinite articles is, very roughly, as follows. We use "the" either when a previous reference has been made, and when "the" signalises that the same reference is being made ; or when, in the absence of a previous indefinite reference, the context (including the hearer's assumed knowledge) is expected to enable the hearer to tell *what* reference is being made. We use "a" either when these conditions are not fulfilled, or when, although a definite reference *could* be made, we wish to keep dark the identity of the individual to whom, or to which, we are referring. This is the *arch* use of such a phrase as "a certain person" or "some one" ; where it could be expanded, not into "some one, but you wouldn't (or I don't) know who" but into "some one, but I'm not telling you who."

(b) *Identification statements.* By this label I intend statements like the following :

- (ia) That is the man who swam the channel twice on one day.
- (iia) Napoleon was the man who ordered the execution of the Duc D'Enghien.

The puzzle about these statements is that their grammatical predicates do not seem to be used in a straightforwardly ascriptive way as are the grammatical predicates of the statements :

- (ib) That man swam the channel twice in one day.
- (iib) Napoleon ordered the execution of the Duc D'Enghien.

But if, in order to avoid blurring the difference between (ia) and (ib) and (iia) and (iib), one says that the phrases which form the grammatical complements of (ia) and (iia) are being used referringly, one becomes puzzled about what is being said in these sentences. We seem then to be referring to the same person twice over and either saying nothing about him and thus making no statement, or identifying him with himself and thus producing a trivial identity.

The bogey of triviality can be dismissed. This only arises for those who think of the object referred to by the use of an expression as its meaning, and thus think of the subject and complement of these sentences as meaning the same because they could be used to refer to the same person.

I think the differences between sentences in the (a) group and sentences in the (b) group can best be understood by considering

the differences between the circumstances in which you would say (ia) and the circumstances in which you would say (ib). You would say (ia) instead of (ib) if you knew or believed that your hearer knew or believed that *some one* had swum the channel twice in one day. You say (ia) when you take your hearer to be in the position of one who can ask : "Who swam the channel twice in one day ?" (And in asking this, he is not saying that anyone did, though his asking it implies—in the relevant sense—that *some one* did.) Such sentences are like answers to such questions. They are better called "identification-statements" than "identities". Sentence (ia) does not assert more or less than sentence (ib). It is just that you say (ia) to a man whom you take to know certain things that you take to be unknown to the man to whom you say (ib).

This is, in the barest essentials, the solution to Russell's puzzle about "denoting phrases" joining by "is"; one of the puzzles which he claims for the Theory of Descriptions the merit of solving.

(c) *The logic of subjects and predicates.* Much of what I have said of the uniquely referring use of expressions can be extended, with suitable modifications, to the non-uniquely referring use of expressions; *i.e.* to some uses of expressions consisting of "the" "all the", "all", "some", "some of the", etc. followed by a noun, qualified or unqualified, in the *plural*; to some uses of "they", "them", "those", "these"; and to conjunctions of names. Expressions of the first kind have a special interest. Roughly speaking, orthodox modern criticism, inspired by mathematical logic, of such traditional doctrines as that of the Square of Opposition and of some of the forms of the syllogism traditionally recognised as valid, rests on the familiar failure to recognise the special sense in which existential assertions may be implied by the referring use of expressions. The universal propositions of the fourfold schedule, it is said, must *either* be given a negatively existential interpretation (*e.g.*, for A, "there are no Xs which are not Ys") *or* they must be interpreted as conjunctions of negatively and positively existential statements of, *e.g.*, the form (for A) "there are no Xs which are not Ys, and there are Xs". The I and O forms are normally given a positively existential interpretation. It is then seen that, whichever of the above alternatives is selected, some of the traditional laws have to be abandoned. The dilemma, however, is a bogus one. If we interpret the propositions of the schedule as neither positively, nor negatively, nor positively *and* negatively, existential, but as sentences such that *the question of whether they are being used to make true or false assertions does not arise except when the existential*

*condition is fulfilled for the subject term*, then all the traditional laws hold good together. And this interpretation is far closer to the most common uses of expressions beginning with "all" and "some" than is any Russellian alternative. For these expressions are most commonly used in the referring way. A literal-minded and childless man asked whether all his children are asleep will certainly not answer "Yes" on the ground that he has none; but nor will he answer "No" on this ground. Since he has no children, the question does not arise. To say this is not to say that I may not use the sentence, "All my children are asleep", with the intention of letting some one know that I have children, or of deceiving him into thinking that I have. Nor is it any weakening of my thesis to concede that singular phrases of the form "the so-and-so" may sometimes be used with a similar purpose. Neither Aristotelian nor Russellian rules give the exact logic of any expression of ordinary language; for ordinary language has no exact logic.

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## IV.—STRONG AND WEAK VERIFICATION II

BY MORRIS LAZEROWITZ

In his introduction to the second edition of *Language, Truth and Logic* Professor A. J. Ayer attempts to meet objections<sup>1</sup> to his view that non-basic empirical propositions are, in principle, only weakly verifiable by resorting to two moves. For one thing, he exchanges the position that basic statements are not propositions for the position that they are empirical propositions which are open to conclusive verification. And for another thing, by holding that basic propositions are strongly verifiable he tries to reinstate the usual distinction between strong and weak verification, or rather he tries to secure the legitimacy of his application of the term "weakly verifiable" to non-basic empirical propositions while withholding application of "strongly verifiable" to them by contending now that the term "strongly verifiable" has an application to basic propositions. To the casual reader these moves might seem to strengthen the paradoxical position that non-basic empirical propositions are at best weakly verifiable, *e.g.*, that the truth-value of the statement "There is a restaurant in the London School of Economics" can be rendered probable but cannot be established conclusively. To more careful readers these moves might seem to be downright mistakes. But if they are, they are very curious mistakes. For they are so transparent that it is not easy to see how a careful thinker could possibly have made them. And it is unsatisfactory to say, "Well, he just made them and that is all there is to it".

The words "transparent", "obvious", "clear" are, to be sure, psychological words which are often used to cajole or intimidate or to make a display of one's mental quickness. There is a story that G. H. Hardy passed over a step in the proof of a mathematical theorem he was lecturing on to his class with the comment, "This is obvious". His students objected that it was not obvious to them, and asked for the explanation. Hardy, who apparently was taken aback by the objection, left the lecture room to look over the omitted step and after a few minutes' deliberation returned and announced to the class, "Yes, it is obvious. We shall go on." Whichever of the three

<sup>1</sup> M. Lazerowitz, "Strong and Weak Verification", MIND, 1939.

motives the tale about Hardy illustrates, my intention in saying that Ayer's mistakes are transparent is none of these. When I say they are obvious I mean that he knows everything necessary in order to know that they are mistakes, but makes them anyway. They are perverse in the way in which all philosophical "mistakes" of any duration seem to be perverse: they go against what everyone knows perfectly well, including the people who make them. And this is something which requires explaining.

My plan in this paper is first to discuss Ayer's views with regard to basic propositions and the distinction between strong and weak verification as if they are mistakes and then proceed to construct a hypothesis according to which they are not mistakes but are unconsciously made revisions of language. It can easily be seen that this hypothesis, though it may come as a shock to many people and lead them to think me a metaphysical Cassandra, has the merit of explaining the perverseness of the "mistakes", of explaining how a person could make mistakes, and persist in thinking them not mistakes, while knowing everything necessary to prevent his making them. For if the views constitute linguistic revisions expressed in the form of statements of fact, then we can see both why they look to be mistakes and also why they can be maintained in the face of known fact against which they seem to go. The points I wish to discuss are in the following quotation:

"To begin with, it will be seen that I distinguish between a 'strong' and a 'weak' sense of the term 'verifiable', and that I explain this distinction by saying that 'a proposition is said to be verifiable in the strong sense of the term, if and only if its truth could be conclusively established in experience', but that 'it is verifiable, in the weak sense, if it is possible for experience to render it probable'. And I then give reasons for deciding that it is only the weak sense of the term that is required by my principle of verification. What I seem, however, to have overlooked is that, as I represent them, these are not two genuine alternatives. For I subsequently go on to argue that all empirical propositions are hypotheses which are continually subject to the test of further experience; and from this it would follow not merely that the truth of any such proposition never was conclusively established but that it never could be; for however strong the evidence is in its favour, there would never be a point at which it was impossible for further experience to go against it. But this would mean that my 'strong' sense of the term 'verifiable' had no possible application, and in that case there was no need for me to qualify the other sense of 'verifiable' as weak; for on my own showing it was the only sense in which any proposition could conceivably be verified.

"If I do not now draw this conclusion, it is because I have come to think that there is a class of empirical propositions of which it is permissible to say that they can be verified conclusively. It is character-

istic of these propositions, which I have elsewhere called 'basic propositions', that they refer solely to the content of a single experience, and what may be said to verify them conclusively is the occurrence of the experience to which they uniquely refer."<sup>1</sup>

It can be seen that Ayer wishes, with a number of other philosophers, to hold that empirical propositions like "There is a restaurant in the London School of Economics" and "The Serpentine is in Hyde Park" are *permanent* hypotheses, which are intrinsically, by their very nature, incapable of being established conclusively. Experience can render them more and more probable, but never certain. No Londoner really knows that the Serpentine is in Hyde Park, and no student at the London School of Economics knows that there is a restaurant there, no matter how many times he has lunched there and argued about the Labour Government. To resort to an analogy, with respect to establishing such propositions conclusively we are in the position of a person who tries to reach the horizon: like him we can get farther and farther away from our starting point in the direction of the goal but the goal itself remains forever out of our reach. Or to use a stricter analogy, we are in the position of a person who tries to arrive at 1 by going out in the series  $\frac{1}{2}, \frac{3}{4}, \frac{7}{8}, \frac{15}{16}, \frac{31}{32}, \frac{63}{64}, \dots$ ; no matter how far he goes out in the series generated by  $\frac{2^n - 1}{2^n}$  an infinity of fractions remains between him and 1.

Regardless of how this view may strike one it is easy to see that the more general view that *all* empirical propositions are only weakly verifiable cannot be maintained. For that would imply that the term "strongly verifiable" applied to no proposition whatever. And this, in turn, would imply that "strongly verifiable" had no literal use, that it was a term without sense. Now, it can be seen that if "strongly verifiable" had no use, then the connected term "weakly verifiable" could have no application to propositions either. For if "strongly verifiable" had no conceivable application to propositions, such that the expression "*p* is strongly verifiable" made no sense, then the term "weakly verifiable" would fail to distinguish in any way between empirical propositions and would become a useless term. The sentence "All empirical propositions are only weakly verifiable empirical propositions", reduces, thus, to the uninformative, pointless sentence "All empirical propositions are only empirical propositions". Like the inhabitants of Emerald City, who experience no colour but green and, consequently, cannot in

<sup>1</sup> *Language, Truth and Logic*, Introduction, pp. 9-10, second edition.

their language *say*, "All things are green", a philosopher who maintains that "strongly verifiable empirical proposition" is a senseless expression rules himself out from being able to say, with literal significance, "All empirical propositions are only weakly verifiable". He cannot say this with sense any more than, as language is at present used, he can say with sense, "All empirical propositions are *meekly* verifiable". Some words are connected to each other in the way in which the wheel of a ship is connected to its rudder, and you cannot destroy the rudder and insist that the wheel has its old function. If "strongly verifiable" is deprived of its use as a contrast term to "weakly verifiable" it is not the case, as Ayer puts it, that there is no *need* to "qualify the other sense of 'verifiable' as weak"; rather there is no other sense to qualify.

Seeing this makes Ayer try to remedy the predicament of his theory in the following way. Basic statements, referring "solely to the content of a single experience", for example, the statements "I have a pain" and "There looks to me to be an elephant in the distance", which formerly he had supposed non-basic and only weakly verifiable,<sup>1</sup> he now holds to be open to conclusive verification, by "the occurrence of the experience to which they uniquely refer". In this way, as he imagines, the required distinction between strong and weak verifiability is reinstated. Allowing the application of "strongly verifiable" to basic statements is plainly designed to prevent the application of the term "weakly verifiable" to non-basic statements from becoming illegitimate. For by allowing a use to the first term he can, it would seem now, distinguish between statements which can be established conclusively and those which experience can render probable only, and so is no longer committed to holding the self-stultifying view that *all* empirical statements are hypotheses. Instead he can now say that some are hypotheses and that others are strongly verifiable.

Two questions have to be discussed. One is the question as to whether it is "permissible" to say that basic propositions are strongly verifiable, or that they can be conclusively established. The other is whether the required distinction between the pair of terms "strongly verifiable" and "weakly verifiable" has been retained by the linguistic manoeuvre of allowing an application of the term "strongly verifiable" to basic propositions. I call what has been done "a manoeuvre with language" because it is designed to protect the main position, which is that non-basic empirical propositions are permanent, or it would

<sup>1</sup> *op. cit.*, pp. 90-93.

perhaps be better to say infinitely permanent, hypotheses. This is the linguistic citadel, whatever the psychological reasons that make holding it important, for the defence of which linguistic outposts are given up.

Let us first consider Ayer's change of mind which makes him grant that it is "permissible" to say that basic propositions are open to strong verification. It is hardly necessary to point out that the verb "verify" denotes a process, the process, namely, of establishing the unknown truth-value of a proposition. By going through the process of verifying a proposition  $p$ , either weakly or strongly, we obtain information about the truth-value of  $p$  which we did not have prior to our going through the process. In other words, the process is one of obtaining new information with regard to the truth-value of  $p$ ; and it makes no sense to speak of verifying what we already know. We can proceed to confirm by observation or an experiment what we believe or what we feel certain of; but to say that we are verifying what we already know is to imply the self-contradictory thing that we do not know what we already know, that we both do and do not know the probability of the truth-value of  $p$ , or that it is certain.

To come now to the question whether it is permissible to apply the term "strongly verifiable" to basic propositions. I can think of no other meaning of the word "permissible", in the context of the present discussion, than "allowed by proper usage". And if "permissible" is being used in this sense, then it is not permissible to apply "strongly verifiable" to basic propositions. It is not English usage to say with regard to any basic proposition that it can be established conclusively in experience, by making tests of any sort or by making introspective observations, because it is not English usage to speak of *establishing* basic propositions. It makes no sense to say, "I have established that I have a pain", any more than it makes sense to say, "I probably have a pain". We should think a person was using language in a queer, improper way if he said, "I probably have a pain, but I have yet to establish this conclusively", and we should think the same thing of a person who said, "I have conclusively verified in experience that I have a pain". It is possible, of course, to suspect a person who claims to have a pain of lying, but it is impossible to think that he might not know whether he has a pain. It is, of course, possible to think that he might not know whether he has a shilling in his pocket, but having a shilling is radically different from having a pain. For a person who has a pain cannot have it

without knowing that he has it. And not only this : his knowledge that he has a pain is not arrived at by a process of verification. It is to be noted that feeling his pain, unlike feeling the shilling in his pocket, does not *establish* that he has a pain. For "I feel a pain" and "I have a pain" mean the same. You cannot have a pain and not feel it or feel it and not have it. There is, thus, no *getting* to know, or conclusively establishing, that you have a pain by feeling it, as if *in addition* to having it, you have to feel the pain in order to know that you have it. Knowledge of the truth of a basic proposition is had *without being arrived at* by a process of verification. By tests you can satisfy yourself that your tooth or your ankle is the cause of your pain, but there are no conceivable tests for establishing that you have a pain.

It is the same with the basic proposition "There looks to me to be an elephant in the distance". By further tests, by rubbing your eyes, getting closer to the place where there appears to you to be an elephant, asking other people, etc., you can satisfy yourself whether you are actually seeing an elephant. But there is no process of establishing that there *looks* to you to be an elephant in the distance. It makes perfectly good sense to say, "What I see is probably an elephant" or "I have conclusively established that what I see is an elephant"; but there is no sense, so far as normal English is concerned, in saying "It is probably the case that it looks to me as if there is an elephant in the distance", or of saying "I have conclusively established that it looks to me as if there is an elephant in the distance". And in general with regard to basic propositions, it is not only the case that we could not say "there would never be a point at which it was impossible for further experience to go against it", but it is also the case that there could be no further experience a person might have, over and above his experience of having a pain or his experience of something appearing to be so and so to him, which would establish for him the fact that he was having the experience.

If Ayer, as it seems, mistakenly thinks that it is permissible to apply "strongly verifiable" to basic propositions, then we shall have to accuse him of confusing two different senses of the word "verifies", of confusing "verifies" in the sense of "makes a proposition true" and "verifies" in the entirely different sense of "establishes a proposition to be true". Ayer's own words give this accusation the semblance of plausibility; for he says, "It is characteristic of these propositions, which I have elsewhere called 'basic propositions', that they refer solely to the content

of a single experience, and what may be said to verify them conclusively is the occurrence of the experience to which they uniquely refer". The experience to which a basic proposition refers, just as the state of affairs to which a non-basic proposition refers, is what makes the proposition true, not the process by which its truth is established. Thus, my having a pain is what makes the proposition that I have a pain true; but what would verify the proposition, if there were any sense in speaking of verifying it, would be an experiment or a series of observations which ended in my knowing that I had a pain. The distinction is made clear in the genuine case of verifying a non-basic proposition. What verifies, in the sense of "makes true", the assertion that a shilling is round is the shape of a shilling; but what verifies, in the sense of "establishes", that a shilling is round is not the shape of a shilling but *experience* of its shape, looking at it, feeling it, etc.

Let us now look at Ayer's second claim, which is that the term "weakly verifiable" has a literally significant application to an entire class of empirical propositions, because, as he thinks, the term "strongly verifiable" is applicable to a different class of empirical propositions. And let us, for the sake of the argument, grant that "strongly verifiable" is meaningfully applicable to basic propositions. The question to decide now is whether, even by allowing this, it can be granted that the proper distinction between the terms has been retained, that is, whether the correct relationship between them is retained by restricting the applicability of "conclusively established in experience" to basic statements and by restricting the applicability of "rendered probable by experience" exclusively to non-basic empirical statements.

In the case of some pairs of terms which function as alternatives to each other, such that the applicability of one of the terms of a given pair depends on the fact that the other has a possible application, the ranges of the terms are mutually exclusive. Thus, consider the designations "even number" and "odd number". These are so related to each other that if "even number" described no number, "odd number" would describe none either. A people whose number system we should, in *our* language, describe as consisting of the odd numbers only would not be able, in *their* language, to say with meaning, "3 is an odd number". But more specifically than this, these designations are exclusive alternatives, in the sense that neither has a theoretically possible application to any number to which the other is significantly applicable. There is no theoretically

possible circumstance in which any even number could, without self-contradiction, be said to be odd, or an odd number even. Or to put the matter with greater care, there is no number with regard to which it would be possible at one time to say, without self-contradiction, that it was even and at a later time to say, without self-contradiction, that it was odd—without, of course, changing language. The same is true for other genuine alternatives: “rational number” and “irrational number”, “*a priori* proposition” and “empirical proposition”.

Ayer apparently conceives the relation between the terms “strongly verifiable” and “weakly verifiable” to be like that between “even number” and “odd number”. It would seem that he thinks the terms are exclusive alternatives, such that under no logically possible condition could a proposition, which at one time is only weakly verifiable, be said to be strongly verifiable. And if this is what he in fact maintains, if he actually holds that he has correctly described the relationship between the terms, as he has defined them, then he has made a mistake. For no proposition can, with sense, at any time be said to be weakly verifiable unless it is, in principle if not in fact, open to strong verification. It is only possible for experience to render an empirical proposition  $p$  probable, to establish it with more or less certainty, if it is possible for experience to establish  $p$  conclusively. There is no sense whatever, so far as established linguistic usage is concerned, in saying that  $p$  can be rendered more or less probable, and therefore to have a greater or lesser approximation to certainty, if it is logically impossible to establish  $p$  conclusively. The linguistic fact, if there is such a fact, which would make it *logically impossible*, as opposed to physically impossible, for  $p$  to be established with certainty, would be the *senselessness* of stating “ $p$  is conclusively established”. And it does not take much thinking to see that if it were really the case that the words “ $p$  is conclusively established” constituted an improper use of language, the words “ $p$  is probable, but less than certain” or the words “The probability of  $p$  is closer to certainty than is the probability of  $q$ ” would also have to constitute an improper use of language—which they do not. For if it made no sense to speak of  $p$  being certain, it would make no sense to speak of  $p$  being less than certain.

It can be seen, thus, that even if we grant that basic propositions are strongly verifiable, it remains impossible to say with sense that non-basic empirical propositions can be rendered probable but cannot, logically, be established conclusively. The attempt to explain the relationship between “strong verification”

and "weak verification" as being like the relationship between "even number" and "odd number" or like that between "*a priori*" and "empirical" fails to make one term a genuine alternative of the other. And also, of course, it cannot actually be admitted that "conclusively established" applies to basic statements. Thus Ayer's two moves are not a satisfactory defence of the view that non-basic empirical statements are permanent hypotheses.

Norman Malcolm, in his excellent paper, "Certainty and Empirical Statements",<sup>1</sup> has argued that the philosophical theory that non-basic empirical statements cannot be known with certainty is mistaken. And I have argued as if the two moves are errors, one consisting of a misapplication of the term "strongly verifiable" and the other consisting of a misconstruction of the proper connexion between the use of "rendered probable by experience" and the use of "established conclusively in experience". Malcolm's claims, like those I have made, are that the theories reduce to mistaken claims about the proper use of language. I think, however, that it is unsatisfactory to view the theories in this way, for doing so throws no light on the perverseness of the mistakes. It gives us no satisfactory explanation of how it could come about that anyone should make such mistakes, and chronically persist in them, while being perfectly well acquainted with the linguistic facts which should have prevented his making them, and should certainly prevent his persisting in them. In ordinary life a person who knows English usage would not, except as a joke, dream of saying to someone who complained of having a pain, "Have you verified conclusively that you have a pain? Have you made sure that you really have one?" He would know that to ask this would not be to ask a question but to do something whimsical with language. Yet the same person in his philosophical talk will insist that it makes good sense and is not a misuse of language to say that we establish, by a process of sense-verification, that we have a pain. It is this sort of discrepancy between what a philosopher knows about proper linguistic usage and how he chronically misdescribes it when doing philosophy that is not satisfactorily explained by supposing that he just makes mistakes.

Freud has explained that an illusion, *i.e.*, a conviction which is had with no evidence for it or one which persists in the presence of confuting evidence, has its source in an unconscious wish. And it seems to me that what I have characterised as Ayer's perverse mistakes can most satisfactorily be explained not as

<sup>1</sup> MIND, 1942.

being mistakes about language but as being changes he unconsciously wishes to make in language. We might say that he is an unconscious linguistic reformer, whose reforms are perhaps not intended for practical adoption. What he does he conceals from himself by the form of speech in which he expresses his re-editing of language. And not only does he achieve concealment by the form of speech he uses ; by it he also creates the wished for illusion that he is doing a sort of science, discovering facts about the nature of empirical propositions.

Consider Ayer's first error of thinking, apparently, that basic, or experiential, statements are strongly verifiable. Earlier he had contended<sup>1</sup> that they were not propositions, and he now describes what he had *really* done in holding this view in the following way : " My reasoning on this point was not in itself incorrect, but I think that I mistook its import. For I seem not to have perceived that what I was really doing was to suggest a motive for refusing to apply the term ' proposition ' to statements that ' directly recorded an immediate experience ' ; and this is a terminological point which is not of any great importance."<sup>2</sup> It requires no great perspicuity to see that what Ayer, in so many words, is telling us is that he was changing language and not simply using it in a mistaken way. His reasoning, as he rightly claims, was not incorrect, for it consisted of no more than a description of the difference between the use of statements that directly record an immediate experience and the use of empirical statements which go " beyond what is immediately given ".<sup>3</sup> When this difference in use impressed him with its importance he *refused* to call any utterance which registered a sense-content a " proposition " ; and when the difference lost its importance for him and a similarity impressed him more his resistance against applying " proposition " to basic statements vanished. Once insight is gained into what was really being done it becomes a " terminological point which is not of any great importance ", but before that it certainly did not seem terminological and was felt to be of considerable importance. To put the matter briefly, the problems about strong and weak verifiability are problems of bringing out and emphasising various similarities and dissimilarities between different kinds of propositions not made explicit, and even concealed, by the usual way we talk about the propositions. If we read in the light of his self-analysis what he writes about the difference between empirical and *a priori* statements, we can realise that in holding statements like " The Serpentine is in

<sup>1</sup> *op. cit.*, pp. 90-92.

<sup>2</sup> *Ibid.*, pp. 10-11.

<sup>3</sup> *Ibid.*, p. 91.

Hyde Park" to be hypotheses he is dissatisfied with the ordinary language used in connexion with them. He writes: ". . . no general proposition whose validity is subject to the test of actual experience can ever be logically certain. No matter how often it is verified in practice, there still remains the possibility that it will be confuted on some future occasion. . . . And this means that no general proposition referring to matter of fact can ever be shown to be necessarily and universally true. It can at best be a probable hypothesis. And this, we shall find, applies not only to general propositions, but to all propositions which have a factual content".<sup>1</sup>

Of course no proposition which is subject to the test of experience can be logically certain, or be shown necessarily true. It is logically impossible for *p* to be empirical and also necessarily true. But what is the point of remarking this except to give a motive for refusing to preface non-basic factual statements with phrases like "It is certain that", "I know that", and in this way marking the difference between logically certain propositions and empirically certain propositions? In ordinary conversation, if a person who had rowed a number of times on the Serpentine and strolled along its banks were asked whether he was certain that the Serpentine is in Hyde Park, he could quite properly answer, "I know that the Serpentine is in Hyde Park". And he might very well say that he was as certain about the Serpentine as he was about  $2 + 3$  being equal to 5.<sup>2</sup> The use of the same language with both ordinary empirical statements and *a priori* ones covers a difference between them felt to be important, and this is a source of dissatisfaction to some philosophers, especially if their respect for mathematics is very great. They then resort to the linguistic expedient, which they restrict to philosophical discourse, of refusing to use "It is certain that" with non-basic empirical statements and, instead, apply "hypothesis" and "weakly verifiable" to them. They know perfectly well that, if it did not indicate a pathological state of mind, it would be an outrage of language for a student who frequently ate in the restaurant of the London School of Economics to say that it is a probable hypothesis that there is a restaurant there. But their *philosophical* view is not an outrage of language; it constitutes a *change* of language for the purpose of bringing out the dissimilarity between logical certainty and empirical certainty and also for the purpose of emphasising the likeness between empirical certainty and probability. The

<sup>1</sup> *op. cit.*, p. 72.

<sup>2</sup> A mathematician once said this sort of thing to me.

difference between the first two is more plainly marked by their new nomenclature as a difference in kind, and the difference between the second two is marked as a difference in degree only.

Once the term "weakly verifiable" is stretched so as to cover all empirical statements which go beyond what is immediately given, it is soon realised, or would be if this new speech were put into practice, that the old distinction between being conclusively established in experience and being rendered probable by experience will have to be reinstated in some way.<sup>1</sup> One way to do this would be to introduce further terminology to do the work of the old. On the highways of New York State there are signs which read "Stop" and others which read "Full Stop". New York automobile regulations apparently recognise two kinds of stops. In a similar way, if we adopted the new terminology, we could mark the ordinary distinction between "establish conclusively in experience" and "render probable by experience" by introducing the expressions "fully weakly verified" and "partly weakly verified", the first to apply to established empirical propositions and the second to those which are rendered only probable.

But Ayer, apparently, is not interested in *working out* the requirements of his new way of speaking. One receives the impression that his revision of language is motivated by emotional rather than by practical considerations, and that one of his objects is to throw statements which go beyond what is immediately given into an unfavourable light. This he achieves, not only by making linguistically more pronounced the difference between them and *a priori* propositions, but also by doing the same thing with regard to the difference between them and basic propositions. He "chooses"<sup>2</sup> to classify basic statements as empirical propositions which are open to *conclusive* verification, and so creates a contrast which is not favourable to non-basic propositions. In its new usage, "conclusively verified", like "weakly verified", no longer has its usual meaning; it no longer has any connexion whatever with verification. In the new way of speaking, *a priori* and basic propositions are to count as being certain and non-basic propositions as being only probable. And the unfavourable light in which non-basic statements are thus thrown is increased by not working out the requirements of the new use of "weakly verifiable". The term

<sup>1</sup> See John Wisdom's "Philosophical Perplexity", *Proc. Arist. Soc.*, 1936-37.

<sup>2</sup> *op. cit.*, p. 11, l. 7.

"strongly verifiable" is not used as the required alternative to the term "weakly verifiable", if the latter is taken to mean what is ordinarily meant by the phrase "could be rendered probable by experience". In their new use the two terms function as genuine alternatives, but not as alternatives which correspond to the pair of everyday expressions, "could be conclusively established in experience" and "could be rendered probable by experience". For "strongly verifiable proposition" means empirical proposition which is not subject to the process of verification, and "weakly verifiable proposition" means empirical proposition which is subject to the process of verification. But Ayer wishes to use the terminology of *strong* and *weak* verifiability for the purpose of creating an unfavourable contrast. And achieving this end is made possible for him by the fact that the terms, in their new use, carry with them former emotional associations.<sup>1</sup>

<sup>1</sup> For a study of the persuasive use of language see Charles Stevenson's "Persuasive Definitions", MIND, 1938.

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## V.—CAN ETHICS DO WITHOUT PROPOSITIONS ?

BY JONATHAN HARRISON

IN this paper I propose to discuss a type of ethical theory that has recently been given much attention. It has been held by Logical Positivists unwilling to class ethical sentences either with those expressing tautologies or with empirically verifiable propositions. But there is no reason why a Logical Positivist must hold it, and no reason why it should not be held by philosophers other than Logical Positivists. It is naturally attractive to a philosopher with a tendency to empiricism and a taste for logical economy,<sup>1</sup> and since it seems to do more justice to the emotional and practical aspects of ethical discussion than the more rationalistic theories against which it is in part a reaction, it has been accepted by some on its own merits.

Forms of it have variously been called the Emotive Theory, the Interjectional Theory, the Expressive Theory, and the Attitude Theory. But in this paper I shall not discuss, save incidentally, any of its species. I shall be concerned only with the genus, and to investigate whether any theory of this type may be satisfactory.

The fundamental difference between the sort of ethical theory we are considering and all others is that, according to it, ethical sentences do not serve to convey information. They do not pass on belief or knowledge about what is or is not the case. They are rather, expressions of the same genus as commands, requests, or ejaculations. In the language of those philosophers who have held the theory, ethical sentences do not express propositions; they do not assert anything. According to one of its forms, I am not, when I tell someone that he ought to keep his promise, expressing a truth about him or the promise or about the act of keeping it. I am just ordering him to keep it. According to another of its forms<sup>2</sup> the ethical statement 'You did wrong to break your promise' does not assert anything more than the purely factual statement 'You broke your promise'. The word 'wrong' serves only to express the speaker's disapproval of

<sup>1</sup> If it is true, there is no need to postulate a special faculty of moral intuition in addition to the faculty whereby we grasp ordinary speculative truths.

<sup>2</sup> A. J. Ayer : *Language, Truth, and Logic*.

such conduct. And according to its most recent form,<sup>1</sup> the Attitude Theory, ethical statements, so far from asserting anything, serve only to express attitudes. To say 'Promises ought to be kept' is not to assert a proposition. It is to express the speaker's favourable attitude to promise keeping, which attitude consists in a disposition to keep promises himself, to encourage others to keep them, and to feel pleased when others do keep them. And it means something like 'If any one has promised anything, let him do what he has promised to do, approve when others do, likewise, and know by these tokens that I, too, will do the same.'

All these theories, though they differ in other ways, agree that talking about morality does not consist in asserting truths but in making communications of another sort. Attractive though they have proved to some, they have provoked from others a storm of objections. These objections almost all arise because it is supposed that certain fundamental distinctions, distinctions which we all draw in ordinary life and conversation, can only be drawn on the assumption that ethical utterances really are a means of asserting something. Since, it is supposed, these distinctions cannot be drawn if there are no ethical propositions, the theory which implies that there are no such things must be rejected.

The distinctions which opponents of the theory claim it cannot draw are these. If ethical words do not serve to tell somebody something, they must be meaningless. But surely there is a distinction between ethical utterances which are meaningful and those which are not. 'He is a good man' is meaningful. 'He is a duty' or 'He is a good' are meaningless. The expression 'Knowledge is an end in itself' is quite clearly meaningful. But whether or not the expressions 'Mrs. Smith is an end in herself', 'Englishmen are ends in themselves', or even 'Human beings are ends in themselves' are meaningful does seem to be a question which admits of doubt.

If we do not use ethical expressions to assert anything, it seems to follow that we cannot argue about morality. If ethical expressions do not express propositions, they cannot express *contradictory* propositions, and it seems to follow that I can neither contradict myself nor anybody else over a moral issue. But clearly people *do* contradict one another about

<sup>1</sup> C. L. Stevenson: *Ethics and Language*. W. H. F. Barnes: *Ethics Without Proposition*. R. Robinson: *The Emotive Theory of Ethics*. The two latter are both published in Volume xxii of the *Aristotelian Society's Supplementary Proceedings*.

such things, and argue about which of them is right. Hence, it is maintained, this theory must be rejected.

Not only do we argue about morality, we give reasons to support our position when we do. Some of these reasons we believe to be good ones ; others we believe are bad ; others we believe are entirely irrelevant. Yet according to the theory there are no ethical propositions for which reasons, good, bad, or irrelevant, can be given.

I sometimes ask myself questions about morals, and answer them one way or another. This involves a passage from a state of doubt to one of certainty. How, on this theory, is this possible ? What can I be doubting, and what can I be believing, if it is not the truth of a proposition ? And on this theory there are no ethical propositions to be doubted or to be believed.

If ethical utterances do not convey information, they cannot convey true or false information. So if two people argue about a moral question, it seems that I cannot think one of them right and the other wrong. For to think somebody right is to believe that the proposition he asserts is true, and to think somebody wrong is to believe that the proposition he asserts is false.

Lastly, what account can this theory give of the possibility of moral progress ? By this I do not mean, 'How can our theory explain how people become better behaved'. I mean 'How can it explain our belief that moral codes may change for the better ?' meaning by 'moral codes' the codes people think they ought to obey, not the ones they actually obey. And it would be beside the point to say that moral progress in this sense never occurs. We have not to explain the fact that moral progress occurs, but our belief that it is not meaningless to say that it does. On the traditional view that a moral code is a set of beliefs, beliefs about what acts are duties and what things are good, our belief in the possibility of moral progress can quite easily be accounted for. Moral progress occurs when beliefs which are false are replaced by beliefs which are true, or at any rate, when beliefs which are far from the truth are replaced by beliefs which are nearer to it. But this way is not open to the theory we are considering, for according to it there are no ethical propositions to be believed, no ethical propositions to be true or false, near to the truth or far from it. What account, then, is it to give of our belief that it is not meaningless to say that moral codes can be improved upon ?

These are very formidable objections, and I do not think that those who hold this theory should underestimate their

force. But before trying to discover to what extent they may be overcome, I want to digress slightly and discuss the accusation that it is a non-moral, perhaps even slightly immoral theory. One distinguished critic has remarked that he believes the ' theory plays into the hands of those demoniac types who have brought the world to its present pass '. And the disturbance and dismay it arouses in the minds of many, though it may well be exaggerated and rest upon misunderstanding, should lead us to suspect that it cannot admit of certain beliefs and distinctions which ordinary men think it important to maintain.

The charge that the theory that there are no ethical propositions ' discredits ethics ', as it has been put, means, I suppose, that it tends to undermine confidence in present moral codes without providing any incentive to search for better ones. And the belief that it does this seems to me to be the result of the common supposition that it cannot answer some of the objections listed earlier. If, for example, no good reasons can be given for accepting one moral code rather than another, why should we accept the moral codes we in fact do ? Why accept any at all, since equally little reason can be given for any of them ? Why maintain my own moral position in argument, if it is not open to me to say that my tenets are true and those of my opponent false ? Why try and improve upon it, if the view that replaces it is no nearer the truth than the view which is replaced ?

I should agree that if many of the objections made above were well founded, this theory would justify this hostile attitude, and would discredit ethics in the sense defined. Whether or not these objections can be maintained I shall discuss later. Meanwhile I shall point out some ways in which the theory certainly does not discredit ethics.

It does not discredit ethics in the sense that it is a view about what things are right, good, and duties which would outrage the moral senses of decent men and women should they come to know about it. Those who hold it do not maintain any *ethical* views, such as that the enjoyment of cruelty is good, that the Fuehrer ought always to be obeyed, that our only duty is to do as we please. By accepting this theory a philosopher does not commit himself to any specifically ethical beliefs at all. As a theory about the status of ethical sentences it is related to theories concerning what things are good and bad in a manner analogous to the way in which an epistemological theory is related to a scientific one. It no more claims to tell us what we ought to do than a theory of truth claims to tell us what propositions are true. Just as a theory of truth alone does not

enable us to decide between the Ptolemaic and Copernican systems of astronomy, an ethical theory such as this is quite neutral between the various views about how we ought to behave. Indeed, it is purely factual, consisting in certain factual beliefs about the nature of ethical utterances and the way moral disputes are settled. And it is commonly, though by no means universally, held that no factual belief maintained in all sincerity can be reprehensible, even if it is mistaken.

Nor does this theory discredit ethics in the sense that it abolishes the distinction between approving something and merely liking it, between feeling that something is a duty, and believing that it is in my interest to do it. Even on the interjectional theory, its most radical and least convincing variety, a difference is noted between expressing approval of something by saying that it is good, and expressing liking, perhaps by saying that it is nice. And a distinction can likewise be drawn between believing that honesty is the best policy, and feeling that men ought to be honest. 'Honesty is the best policy' expresses a belief about the effects of honesty on the wealth and happiness of those who practise it. 'Men ought to be honest' expresses the speaker's attitude to promise-keeping, an attitude which our theory may hold to be of a peculiar and distinctively moral kind. Our theory can even draw, as ordinary subjective naturalism cannot, a distinction between approving something and feeling that it is worthy of approval. To say that something is approved is to express a belief *about* an attitude or an emotion; to say that something is worthy of approval is to express this attitude or to evince this emotion. To express an attitude or emotion and to believe that I have it are no more than same thing than to say 'I am annoyed because I have lost my collar stud' is to swear in consequence of losing it.

To this extent the theory is certainly not an immoral or even non-moral theory. To see whether it is in any respect such we must return and consider whether the objections mentioned above really do apply to it. In doing so I shall not attempt to elaborate a form of it which I think will escape them. I shall just enquire whether certain distinctions, necessary to ethics, and which can be made concerning propositions, can be made concerning communications of other sorts. If they *cannot* be made, then the theory must be rejected, but it does not follow that if they *can* be made, it must be accepted.

The first objection, that the theory does not admit of a distinction between meaningful and meaningless ethical utterances, need not detain us long. There are more sorts of meaningful

communication than those which convey true or false information. The expression 'Get up and eat your breakfast' means something quite different from 'Go to bed this minute', yet neither of them communicate belief, and neither could meaningfully be said to be true or false. Similarly, sentences which express commands mean something different from those which express requests, and both have a different meaning from those which ask questions. Even ejaculations, the type of utterance which I suppose comes nearest to being meaningless, mean different things. 'Alas' does not mean the same as 'Hurrah', nor 'Ouch' the same as 'Dear me'. It is true that there are senses in which these latter expressions *are* meaningless. They are meaningless in the sense that they do not name an object, or a class of objects, or a characteristic common and peculiar to a class. But words can have meaning in more ways than one, and because a word is meaningless in one sense, it does not follow that it is meaningless in all senses. You might almost as well argue that the word 'nothing' is meaningless because it does not name a class of objects which are nothing, united by their possession in common of the attribute 'nothingness'.

The second objection resolves itself into two parts. It is supposed to be impossible for ethical communications to be inconsistent with one another on this theory. And it is further supposed to be impossible for it to explain how dispute on moral questions ever takes place.

Now commands, though not a species of proposition, and incapable of being true or false, can, nevertheless, be inconsistent with one another. The command 'Halt' is inconsistent with the command 'Quick march', and I would be incapable of obeying both simultaneously. The command 'Go away' is contradicted, or perhaps I should say countermanaged, by the command 'Don't go away'. Commands can even be arranged into pairs which have properties similar to the pairs of contradictories, contraries, and sub-contraries in the traditional square of opposition. It is true that if we apply the words 'contrary', 'sub-contrary', and 'contradictory' to commands, we cannot define them in the same way as we do when talking about propositions. Two propositions are usually said to be contraries, for example, if they cannot both be true, but can both be false. Commands cannot be contrary in this sense, for true and false cannot meaningfully be predicated of them. Similarly, when I say that two commands are inconsistent, I cannot mean that they may not both be true. But this does not show that commands cannot be inconsistent with one another. It just

shows that the word 'inconsistent' is used by logicians in rather a narrow sense.<sup>1</sup>

If commands can be inconsistent with one another, although they are not propositions, so can ethical communications be inconsistent with one another, even if they are not propositions. And not only may there be inconsistent commands. There may be categorical commands ('Take your umbrella'), hypothetical commands ('If it rains, take your umbrella') and disjunctive commands ('Either take your umbrella or wear a mackintosh'). Corresponding to hypothetical and disjunctive commands, there may be hypothetical and disjunctive 'command-arguments'. The hypothetical command 'Don't get up if you feel tired' and 'I do feel tired' together involve the categorical command 'Don't get up.' If from 'Don't get up if you are tired' and 'I am not tired' I understood the command 'Don't get up' to apply, my thinking would be invalid. The proper understanding of commands even has its analogue to syllogistic argument in the proper understanding of propositions. If an undergraduate reads a notice saying 'Gentlemen are requested', supplies the missing premise 'I am a gentleman' and understands that the order applies to him, he is indulging in syllogistic reasoning *of a sort*, though it is not a sort of reasoning which involves propositions alone.<sup>2</sup>

<sup>1</sup> Mr. Peters (MIND, lviii, p. 539) says of the word 'contradict', 'I think it is well established that we only use this verb in connexion with assertions, and with sentences used to make assertions'. But has Mr. Peters never come across the expression 'issuing contradictory orders'?

I feel that it would be to misuse words like 'contrary' and 'sub-contrary' to some extent to apply them to commands. But there is *some* similarity between the relation between 'All of you go' and 'None of you go', and between 'All of them went' and 'None of them went'. Calling both these pairs of expressions contraries serves to bring out this similarity —though Mr. Peters is right, and it would also obscure differences.

<sup>2</sup> Mr. Peters, this time I think rightly, objects to the use of the word 'reasoning' as applied to this type of activity. Nevertheless, it is like reasoning, and it is important that this should be recognised.

He also points out that if his wife tells him to polish the floor or wipe the dishes, he cannot infer that she commands him to polish the floor. This is quite true. But what Mr. Peters does not realise is that if she asserted that he was either stupid or disobedient, and then asserted that he was not stupid, he would be *equally* wrong if he inferred that she had asserted that he was disobedient. Nevertheless, the proposition that he was disobedient would be entailed by her assertions, even though she had not asserted it—as, I suspect, the command 'Polish the floor' is involved by her commands, even if she has not ordered it.

Recognition that some commands are involved by others may not be inference—though I think it is in some respects *like* inference—but it is an activity over and above drawing inferences from the *facts* that commands

If, then, there can be something like valid reasoning concerning commands, it does not seem necessary to hold that ethical utterances express propositions to explain how there can be valid reasoning concerning them. Ethical thinking, in other words, is not entirely *sui generis*, even on the assumption that there are no such things as ethical propositions. In it the apparatus of ordinary thought is employed, in part at least.

This theory, then, does admit of a distinction between consistency and inconsistency in ethics. Does it allow the possibility of argument on moral issues? If all argument consists in the assertion by one person of what another denies, then it clearly cannot, for it holds that there are no ethical propositions to be asserted or denied. But there are sorts of argument other than this, and perhaps the theory can find a place of argument about morality among them.

Consider, for example, the following Argument.

- A. You're afraid.
- B. I'm not.
- A. You are afraid.
- B. I'm not.
- A. You are.

and so on. Here one of the things at issue between the parties is the truth of a proposition, though, no doubt, A is not asserting it because he believes it to be true so much as to incite B to make it false. But consider, by way of contrast, this example.

- A. Let me have it.
- B. No.
- A. Give it me.
- B. I won't.
- A. Please do.

and so on. Here the point at stake is plainly not the truth of a proposition but the performance of an action. The dispute will not be settled by the acceptance by B of an assertion put forward by A, but by the performance by B of an action which A demands.

Argument, then, may be interpreted a good deal more widely than some philosophers who have objected to the theory seem to suppose.<sup>1</sup> Ethical dispute may resemble that which takes

have been made. Mr Peters seems to recognise only this latter sort of activity. However, I do agree with him to the extent that it is important not to press the parallel between reasoning in propositions and interpreting commands too far. Perhaps one of the most important differences is that there does not seem to be anything analogous to the law of the excluded middle where commands are concerned.

<sup>1</sup> Mr. Peters would apparently refuse to call the latter type of dispute

place in our second example more closely than that which takes place in our first. It is not necessary to hold that one party in a dispute about morality is asserting something that the other party will not accept. He may be exerting pressure which the other party is resisting.

Neither is the fact that in ethical argument reasons may be given, and that these reasons may be good or bad, an insuperable objection to the theory. Reasons may be given for things other than the truth of a belief. Reasons may be given for doing something, for coming to a decision about what to do in the future, for holding a preference, or for adopting a settled principle of action. Supporting our ethical position by giving reason for it, our theory must hold, is more like giving reasons for reading philosophy, or for preferring golf to tennis, than it is like giving reason for believing that the world is flat or that there are no men on Mars. And just as good reasons may be given for reading philosophy as well as bad, so it is possible to support our ethical position by good reasons as well as bad, even if to defend an ethical position is not the same thing as to defend a belief.

Just what constitutes a good reason for an ethical view ? Given that I do support my position in an ethical argument by giving reasons for it, and that these reasons together with the position which they are put forward to support constitute an ethical argument, which has thus both 'premises' and 'conclusion', what connexion must subsist between premise and conclusion if the argument is to be valid ?

It is not enough to say that ethical arguments have a criterion of validity which is neither that of deductive logic nor that of scientific method. This is doubtless true, but it does not serve to distinguish the theory from the intuitionism of philosophers like Sir David Ross, who also think that ethical arguments are in this respect unique.<sup>1</sup> And it is, furthermore, too negative a statement, since he says "I have always understood that to argue is to maintain by reasoning, or to prove". The parties to the latter dispute are quite clearly not proving or maintaining by reasoning anything; but surely he is mistaken in thinking that the word 'argument', either is or should be used in a way as narrow as he suggests.

<sup>1</sup> See *Fallacies in Moral Philosophy* by Stuart Hampshire, in *MIND*, lviii, p. 470. Mr. Hampshire seems to imply that philosophers of this school have thought that no moral statements could be 'defended or refuted by appeal to the empirical sciences'. In this I think he is mistaken. Such philosophers would certainly hold that the *factual* statement 'I am indebted to Mr. Jones' was relevant to the *ethical* statement 'I ought to lend Mr. Jones the money he is asking me for'—and they would agree with Mr. Hampshire that it was a funny sort of relevance. It is

description of ethical argument. It is not sufficient to say what a good ethical argument is *not* like ; it is necessary to give some positive account of what an ethical argument *is* like. In doing this, we must explain in what way it is possible for an ethical argument to be a good one, although its 'conclusion' is not a proposition. Thus we can neither say that its 'conclusion' follows from its 'premises', nor that its 'premises' make probable its 'conclusion', for both these statements would imply that the 'conclusions' of ethical arguments were propositions. Supposing I am debating with myself whether or not I should go to a certain meeting. The argument by which I convince myself that I ought may run 'I promised I would, and everybody ought to keep their promises'. This argument is of a sort analogous to ordinary deductive argument. I cannot consistently hold that all promises ought to be kept and at the same time hold that this promise ought to be broken. This sort of argument, therefore, presents no difficulty for the theory. Ordinary criteria of consistency and inconsistency apply to commands as to propositions, and so the fact that they apply to ethical argument does not show that ethical arguments are built up of propositions.

But though criteria of consistency alone may prevent me from holding both that all promises ought to be kept and that this promise ought to be broken, they cannot alone prevent me from holding that all promises ought not to be kept, or that this action would not be the keeping of a promise. So ethical views cannot be established by considerations of consistency, *i.e.* of form alone. What other conditions must hold for the reasons put forward in support of an ethical view to be good ones ?

The propositions put forward to establish ethical principles will be propositions about facts.<sup>1</sup> If they are to succeed in statements saying *what* facts are relevant to ethical statements, *e.g.* "I ought always, or almost always, to keep my promises and pay my debts", that they would think could *not* be established or upset by empirical methods. They are right about this, for if such a statement could be established by an appeal to fact, it must be possible to make a further statement asserting that these facts are relevant to *it* ; and this latter statement cannot be established by appeal to fact, for otherwise an infinite regress would result.

<sup>1</sup> By facts, I do mean *all* the facts, whether psychological, sociological, historical, metaphysical, or religious, not just what Mr. Hampshire calls immediate facts. Mr Hampshire suggests that some philosophers have only recognised the relevance of what he calls immediate facts, and have therefore supposed that disagreements which could be settled by appeal to facts which were *not* immediate could not be settled by an appeal to facts at all. He has not convinced me that any of the philosophers about whom I presume he is talking have actually done this.

establishing an ethical principle, they really must be facts, *i.e.* *true* propositions about facts. And much ethical argument is dispute about what is actually the facts. This presents no special difficulty for the theory, since such disputes are just like any other. Not only must they be facts, however; they must be *all* the *relevant* facts. But it is no use regarding this as a complete answer, for part of the problem we are trying to solve is 'what is a relevant fact?'

I think the theory would have to say something like this. A fact is a relevant fact if it actually has a tendency to show that some action or state of affairs is of a sort which I approve or disapprove, or which has consequences which I approve or disapprove. Thus what facts are relevant will depend upon the feelings and attitudes of the person who is adducing them. If he is arguing with someone else, it will depend also upon the feelings of his opponent. And so what is relevant for one person will not be relevant for another, because they may feel differently about different things. One person may produce reasons which convince him that a certain action is right, but which will not convince his opponent—not because his opponent does not think they are facts, nor because his opponent thinks there are other facts which have not been mentioned, but simply because he does not think that they are *relevant* facts.<sup>1</sup> Thus a relevant fact, on this theory, will be something like an emotionally effective fact, and a good argument one the 'premises' of which are facts, which are emotionally effective facts, and which are all the emotionally effective facts. Since what emotionally affects one person will not emotionally affect another, the criterion of what constitutes a good argument will be to this extent subjective.

So much for ethical argument. The theory can also, I think, explain the passage from doubt to certainty which takes place in the mind of a man pursuing an ethical enquiry without having to postulate ethical propositions which he begins by doubting,

<sup>1</sup> Mr. Hampshire (*MIND*, Iciii, pp. 477-478) thinks that such " 'ultimate' moral differences" may occur, but thinks that the people differing, since they agree about the facts, must be using ethical words in different senses. But if this is really the case, how can they be disagreeing with one another? They may think they are disagreeing with one another, but this must be because they are confused by the ambiguity of the words they are using. Mr. Hampshire thinks he has explained how it is that two people can use the word 'ought' in different senses, and nevertheless really be disagreeing with one another, when he says ". . . their judgments contradict each other in the sense in which two conflicting commands

:recommendations may be said to contradict each other". But personally I cannot see how two apparently contradictory commands can really conflict unless the words by which they are expressed are being used in the *same* sense in each.

and of which he ends by being certain. Doubt concerning morality, it must hold, is not like doubt concerning facts. It is more like hesitating between a number of alternative courses of action. And certainty concerning morality, it must hold, is not like being confident in the truth of a belief. It is more like being quite decided which of a number of actions to perform.

The theory can also, I think, account for the fact that when two people argue about morality, I can agree with one of them and disagree with the other. What it must not do is to interpret agreement in morality as consisting in the sharing of a belief. Supposing that Mr. Jones holds that promises ought always to be kept, while Mr. Smith holds that sometimes they ought to be broken, and suppose, furthermore, that I agree with Mr. Smith. I might say that my statement 'Mr. Smith is right' means 'I share Mr. Smith's attitude to promise-keeping'. In this case it should be remembered that the sentence 'I share Mr. Smith's attitude' will express different propositions when uttered by different people, and that some of these propositions will be true and some false. So on this analysis, the statement 'Mr. Smith is right' will be true or false according to who makes it, and is in this sense subjective. If this is regarded as a difficulty, it might be escaped in the following way. 'Mr. Smith is right', it might be held, just means 'Promises *ought* sometimes to be broken'. Here I am not saying that Mr. Smith's attitude to promise keeping and mine are similar. I am merely expressing my attitude, which attitude I know Mr. Smith shares. On neither account am I asserting that the proposition he believes is true.

Those who hold the theory might attempt to find a similar meaning for sentences which state that moral progress has occurred. The problem was, you will remember, to explain how moral progress could take place if there were no false ethical beliefs to replace, and no true ethical beliefs to replace them. This problem might be solved something like this. My belief that Mr. McDonald's moral code has improved might be analysed 'Some time time ago Mr. McDonald approved of eating missionaries. Now he does so no longer. His present attitude to missionary-eating approximates more closely to mine than the one he used to have'. Here it should be remembered that the latter sentence would express a *false* proposition if it were uttered by one of Mr. McDonald's unregenerate relatives. A more simple type of analysis, however, would be one like this. 'Mr. McDonald no longer approves of eating missionaries. And eating missionaries is wrong'.

The theory might give a similar analysis of the belief that my own moral code has improved. Here is another sample analysis. 'When I was younger I disapproved of working on Sunday. Now I don't. And it is not wrong to work on Sunday'. Here the use of the word 'not' does not imply that the sentence 'It is wrong to work on Sunday' expresses a proposition which is false, any more than the use of the word 'not' in the expression 'Do not walk on the grass' implies that 'Do walk on the grass' expresses a proposition which is false.

This theory can, then, give some sort of account of moral progress without implying that it is progress towards greater knowledge. But we do not think that our moral codes always change for the better. Sometimes, we think, they may change for the worse. What meaning is the theory going to attach to our belief that this is possible, if it is not to say that there are true ethical propositions which are changed for false ones.

It can deal with our belief that *other people's* moral codes may change for the worse in a manner precisely analogous to the way in which it dealt with our belief that other people's moral codes may change for the better. My belief that Mr. Jones' moral code has changed for the worse, for example, might be analysed after this manner. 'In his youth Mr. Jones disapproved of gambling, smoking and drinking. Now he no longer disapproves of these things. I share his former disapproval of gambling, smoking, and drinking. These things *are* wrong'.

But what can the theory do about the belief that our *own* moral code may be a change for the worse, that it may actually be more mistaken than the one I abandoned to make place for it? It can point out, quite justly, that it is impossible to believe that the moral code I hold now ever *is* a change for the worse. I can no more believe this than I can believe that the *theoretical* beliefs I hold now are wrong, while the ones they replaced are right. I must believe the things I believe *now*, so I can't believe that they are false while the ones they replaced are true. Similarly, I must take up the moral attitudes I have now and judge my former attitudes from their standpoint. So where my former attitudes diverge from my present ones, I must hold that it is my former ones that are mistaken. It would be a logical impossibility to believe otherwise.

But this theory cannot escape the objection this way. It is, certainly, impossible to believe that my present moral attitude *is* mistaken, or more mistaken than my previous one. But it is not impossible to believe that it *may* be. I cannot at the moment see what account the theory *can* give of this belief. To believe

that my present moral tenets may be mistaken cannot be to believe that other people do not share them. I may *know* that other people do not share them, and at the same time only *think* that they may be mistaken. Nor can it be to believe that I may change them. For I can entertain the possibility that my belief that they may change is false, while my belief that they are mistaken is not; whereas if these beliefs were identical, I should *not* be able to do this.

For these reasons I find it difficult to see how the theory we are discussing can give a satisfactory account of our belief that our present moral tenets may be mistaken. It cannot explain how I can change my attitude to a moral issue, and at the same time believe that I may not be progressing.

That our theory can give a satisfactory account of this seems the inevitable consequence of believing, as does Professor Barnes,<sup>1</sup> for example, that attitudes are the final court of appeal on moral questions. On this theory corrigibility is admitted to a certain limited extent. My tenet 'All promises ought to be kept' may be specified as a result of reflection until it becomes 'only certain sorts of promises ought to be kept'. And our attitude to lying may be modified and corrected in the light of our attitude to causing unnecessary pain. But once our attitudes have been rendered quite self-consistent and determinate in this way, there is, apparently, no further room for error. And to say that there is no further room for error is not just to say that our attitudes never do err in fact. It is to say that it does not even make sense to raise the question whether they are mistaken or not. And since it does seem to make sense to raise this question, our theory is faced with a difficulty.

The force of this objection must not, however, be overrated. It may be that when I believe that attitudes may change for the worse I may just be believing that other people's attitudes may change for the worse, or that my own attitudes may have changed for the worse in the past. And it may be, after all, that the theory can consistently allow us this belief. If it can, I hope those who hold it will explain how, for it seems to me that its inability to do this is its most serious defect as compared with intuitionism. Lastly, it may be that, in putting forward this objection, I, too, am haunted by that 'ghost of the doctrine of Divine Commandments sitting crowned upon the grave thereof' to which Professor Barnes attributes the illusion of Ethical objectivity.

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<sup>1</sup> W. H. F. Barnes: *Ethics Without Propositions*.

## VI.—DISCUSSIONS

### PROFESSOR RHINE'S VIEWS ABOUT PK

In the July 1949 issue of *MIND*, Professor Price gave us a most interesting review of Professor Rhine's latest book, *The Reach of the Mind*, and may thereby have stimulated many others to read it. Professor Price devoted the greater part of his review to a discussion of the PK experiments and their interpretation. It may be worth while to draw attention to certain reasons why the facts, as they are described in this book, fail to warrant Rhine's interpretation of them. It is not proposed here to enter into metaphysical controversy, but merely to indicate two alternative explanations of his experimental results which had not, so far as one can gather from this book, been eliminated in the Duke laboratories. This purpose can be most simply fulfilled if we use language appropriate to the psycho-physical dualism which Rhine accepts.

Professor Rhine has argued<sup>1</sup> that, when we admit the possibility of precognition, what purports to be "pure telepathy" can often, if not always, be explained solely by reference to "pure clairvoyance". Judging by what Rhine says in this book, he does not, however, seem to have realised that some, perhaps all, of the extra-chance correlations between dice-falls and volitions might be explicable solely by reference to ESP. Before any one is entitled to conclude that PK is a distinct supernormal phenomenon, he has to eliminate the following hypothesis (1) as a possible explanation of the dice-throwing results:—

The person who selects the target to be willed by the "agent" in the dice-throwing experiments may (unconsciously) precognise what face (or group of faces in high- and low-dice calling) is going to preponderate above chance-average in the next throw or run, set or series, and his choice of targets (including decisions when to change targets) may be influenced by this supernormally acquired information. In many of Rhine's experiments it seems to have been left to the "agent" to select the target. But even if this is done by an experimenter, we may assume that this experimenter hopes that the test will succeed, so that a motive would exist to account for the direction of his precognitive faculty in this way and for the use to which, we are supposing, he (unconsciously) puts it.

The conditions of some of Rhine's experiments may in fact have eliminated this hypothesis, but, if so, it is unfortunate that Rhine did not make it clear that (and how) it was eliminated. What we need to know, but are not told by Rhine, is the precise methods by which targets were selected. He does of course tell us that, in many

<sup>1</sup> *The Reach of the Mind*, pp. 66-68.

series, "high dice" and "low dice" were selected for equal numbers of throws—but, while this eliminates dice-bias, it contributes nothing towards eliminating our present hypothesis.

The chart on page 84, showing declines within sets, does not tell us enough to eliminate this hypothesis. Rhine tells us "there was need for frequent changes in test procedure if it (the scoring) was to be kept above chance average . . . after a time therefore we adopted a general rule to make some change after every two or three runs". Rhine does not tell us whether *one* of the changes regularly made between sets was to change the target; if this was so, then the results would appear explicable solely in terms of ESP, on the assumption that the selection of targets was influenced by precognition of the face(s) which was/were to preponderate above chance-average in the *first* run of the next set: for almost all of the extra-chance hits occurred in first runs of the sets, and Rhine does not give us the data by which we might calculate whether the results obtained in the remainder of the sets are statistically significant.

If Rhine can show us that the declines within sets and within runs were independent of changes of target, this would probably eliminate the present hypothesis to the satisfaction of most people. One (perhaps the ideal) method of eliminating this hypothesis would be to have the targets, which the agent must will, selected in a random way *by a machine*. In that case, if precognition of the face(s) which will preponderate above chance-average occurs in any mind, this mind will not be able to use this information in order to effect extra-chance scoring *without exercising control by PK over the target-selecting machine*.

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Rhine's assumptions about the *modus operandi* of PK in dice throwing are open to question in several respects: one of these—the assumption that PK implies ESP to guide it—has already been challenged by Professors Broad<sup>1</sup> and Price.<sup>2</sup> Rhine's assumptions must be challenged in another respect. Rhine seems to take it for granted that the dice throwing results must be attributed to a psychic force competing successfully with the laws of gravity at some period(s) during the dice's fall.<sup>3</sup> But so far as one can judge from the descriptions of experimental conditions given in *The Reach of the Mind*, Rhine has not shown that the significant scoring requires us to assume that there are at work in the inorganic sphere any forces not already acknowledged by the physicist. We are told that in many experiments the agent performs some manual movement which

<sup>1</sup> *Vide S.P.R. Proc.*, Part 172, Vol. xlviii p. 22.

<sup>2</sup> *Vide MIND*, Vol. Iviii, No. 231, p. 392 *et seq.*

<sup>3</sup> *Vide The Reach of the Mind*, Ch. 7, e.g. "the psychical energy of PK along with the other forces in the rolling die is converted into resultant kinetic energy that effects the placement of the dice". (P. 97.)

constitutes a differential condition of the subsequent movements of the dice ; we are not told whether there have been any experiments in which the agent performs no such movement. Hence an alternative hypothesis (2) may be entertained :—The extra-chance scoring may be due to control (normal or abnormal) by the agent of the processes in his own body which "trigger" the movements of the dice.

Hypothesis 2 may appear at first to be merely the repetition of an obvious criticism. It has often been suggested for example that, when dice are hand-thrown, some people may be able to get a preponderance of the results they wish by means of a normally acquired manual dexterity. Hypothesis 2 is intended, however, to put us on our guard against possibilities which are more unexpected. Psychical researchers have treated seriously the possibility of hyperaesthesia in connection with ESP. Experimenters have recognised that they ought to consider whether and to what extent ostensible ESP may be mediated by processes occurring in the afferent nervous system, assuming abnormal sensitiveness and discrimination on the part of the senses : they ought equally to consider whether and to what extent ostensible PK may be mediated by processes occurring in the efferent nervous system, assuming abnormal control of such processes. The precautions to eliminate hypothesis 2 in PK experiments should be as rigorous as the precautions to eliminate hyperaesthesia in ESP experiments. Let us consider how hypothesis 2 might be applied to the Duke experiments on the basis of what we are told in *The Reach of the Mind*.

Take the conditions of mechanical release mentioned by Rhine at top of page 82. For the present purpose (isolating one variable differential condition) this seems the simplest case. In this case the physical factors at the moment of release would comprise :—

(A) Static conditions (i) the precise position of each die, (ii) the precise position of each surface which each die will strike in falling.

(B) A differential condition—a movement of the body which moves to release the dice.

Now this movement (since it was manually initiated) must have been a variable as between different "throws"—a variable in respect of speed and direction. It may seem difficult to believe that an agent could control the speed and/or direction of the release-movement so as to achieve a willed result ; but it seems much more difficult to understand how a psychic force could deflect a moving cube !

This hypothesis is of some theoretical importance for it would enable us to accept Rhine's facts, without regarding any mental events as *proximate* differential conditions of any physical events other than those occurring in the corresponding brain. Assuming that volitions are *remote* causal ancestors of some extra-chance hits, they would be performing this rôle "by normal channels", *i.e.* via brain, efferent nerves and muscles.

The old mystery of how mental events can control cerebral events would remain, would indeed be heightened by virtue of the discrimination which, upon this hypothesis, we should be attributing to the causal efficacy of volitions. However, the physicist who ignores *this* problem (how cerebral events are determined) could, on our present hypothesis, still maintain (a) that we need invoke no causal connexions between events which are not linked by a spatio-temporally continuous series of events, and (b) that all of the necessary conditions of any physical event are contained in prior physical events and states. (I don't know whether the physicists have decided whether they want to maintain these propositions. They seem, when dealing with the Quantum theory, to be abandoning both the principle of Continuity and Determinism in the "microscopic sphere", yet they seem to wish to retain both in the "macroscopic sphere".)

In indicating why the Duke experiments are compatible with this hypothesis, we have considered only one respect in which the agent's bodily movement may constitute a variable differential condition of the dice-movements. Differently designed experiments may leave open different opportunities of this kind. For example, a different kind of opportunity would occur if the agent manually replaces any of the dice in the release-mechanism, thus determining the spatial arrangement of the dice at the moment of release. Another kind of opportunity would occur in the "rotating cage" experiment where the agent initiates release by pulling a string.<sup>1</sup> In this case, even if the speed and direction of the agent's pull were causally irrelevant, the timing of the pull might be relevant. In the rotating cage, the dice are presumably in motion relative to each other and to the cage and to the table. At one instant the spatial distribution and motion of the dice will be such that their release *then* would give an excess of high dice: at another instant . . . an excess of low dice. Significant scoring with this set-up might be attributed to the agent's abnormal control of the timing of his motor behaviour.

If this hypothesis is true, (some) human beings have an unexpected faculty, but one which is not all like PK as Rhine conceives the latter. We may note here that Rhine's description of PK, in terms of a psychic energy or force which competes with gravity, does not seem to be essential to his thought. The most important feature of PK as he conceives it seems to be the occurrence of direct causal influence of a mental event on extra-somatic physical events ("direct" meaning "not mediated by any physical process in the associated organism"). Let us use "PK" to refer only to the occurrence of such causation. Can we eliminate hypothesis 2? Can a person's volition influence the course of events in external bodies except by way of motor processes (or other physical processes of known kinds) in his own body? It seems easy to eliminate *the agent's* bodily movements qua differential conditions of the events which he seeks

<sup>1</sup> *The Reach of the Mind*, p. 95.

to influence. We simply debar the agent from replacing dice in the release mechanism and from initiating the release movement. But this is not enough. On the hypotheses in question *agents* have unexpected powers of controlling their movements—then presumably *experimenters* may have like powers—so that *any* person who manually replaces the dice or initiates the release-movement might be responsible for the excess-hits. (In the Duke experiments, the experimenters know the target, and, we may assume, are “willing”—whether consciously or not—that the experiment will succeed: and even if the experimenters did not know the target by normal means, they might ascertain it by ESP). We require therefore an experiment in which all of the events, which the physicist regards as differential conditions of the events which the agent seeks to influence, are determined by machines.

Fortunately for the progress of psychical research, it does seem possible to design PK experiments by which hypotheses 1 and 2 can be eliminated or confirmed. The writer has suggested elsewhere<sup>1</sup> one experimental design, using electronic gadgets, which would serve this purpose. But, until hypotheses 1 and 2 have been eliminated, the facts will not compel any one to admit that a mental event is ever a *proximate* differential condition of any physical event occurring outside the corresponding organism.

Some people may consider it idle to entertain hypothesis 1 as an alternative to the PK explanation, on the grounds that it is just as fantastic to postulate precognition as to postulate PK. With such critics rests the onus of explaining otherwise the experimental results of Dr. S. G. Soal,<sup>2</sup> Whately Carington<sup>3</sup> and others. But whatever one's views about the evidence for precognition, one might reasonably expect Rhine to try to eliminate hypothesis 1, since he argued<sup>4</sup> that the occurrence of precognition was established by some experiments performed at Duke; and since moreover he considered the possibility that the results of these experiments might be explained in terms of PK without invoking precognition, and eliminated this latter possibility to his (provisional) satisfaction. Let us consider how Rhine does this.

A subject has to guess what will be the order of a pack of cards after it has been shuffled at a later date. In order to eliminate the possibility that the subject's guesses might influence the final order of the pack by influencing the mind and hence the fingers of the person who shuffles the pack, Rhine arranged for the shuffling to be done by machine. He then envisaged the possibility that the subject's guesses might exert a PK influence on the shuffling machine. Rhine's ingenious attempt to eliminate this possibility may be given in his own words.

<sup>1</sup> *The Experimental Evidence for PK and Precognition* (to be published in *SPR Proc.*)

<sup>2</sup> *S.P.R. Proc.*, Part 167.

<sup>3</sup> *S.P.R. Proc.*, Part 162.

<sup>4</sup> *The Reach of the Mind*, pp. 60-64.

"This new demand was far from easy to meet. The cards had to be randomised by some method beyond human control, even beyond the possibility of direct mental action on them through psycho-kinesis. There may be some question whether we have met the demand yet, but I think we have. In order to rule out any direct mental influence on the fall of the cards in the shuffling machine we went back to Nature herself, and decided to cut the pack of cards on the basis of the daily figures for the temperature extremes obtained from a specified newspaper for a given day fixed in advance. A routine way of using the numbers was worked out and agreed upon. This proceeding left nothing to chance or human influence, unless the temperature or its recording device could be subject to such control. On this basis Dr. Humphrey and I have carried out two experimental series of precognition tests at the Duke Laboratory, both of which were statistically significant. So the precognition hypothesis survived another crisis. And for the present at least, there is no 'difficulty number *three*' in sight to challenge its claim as an established phenomenon of nature."

A third difficulty may however be raised. Rhine's experiment seems to prove precognition on the grounds that if you dispense with precognition at one point in your explanation of the facts, nevertheless you must invoke precognition at another point: to be more explicit—if you attempt to explain the results *without* invoking direct precognition of the order of the pack after shuffling and cutting, but by invoking instead a PK influence on the mechanical shuffler, nevertheless you must on this latter theory postulate precognition of the future temperature readings which determine the way in which the pack is cut, for without this last information the PK influence on the shuffling machine could not be guided so as to effect extra-chance scoring. But this conclusion follows only if we assume that PK influence must be guided by information supernormally (or normally), acquired. If we agree with Professors Broad and Price that we are not entitled to assume that PK *must* depend on ESP, then Rhine's experiments are not unambiguous evidence of precognition. This conclusion may, in any case, be reached by a simpler route: for we ought surely to ask why Rhine should assume that thermometers differ from card-shuffling and dice-throwing machines in being immune from PK influence. Rhine's culminating experiment fails in its purpose unless he can show that thermometers must or do enjoy this privilege. Rhine does not attempt to do this—presumably because he was thinking of the temperature readings as being determined by Nature herself.

We ought surely to conclude that Rhine's above quoted experiment provides evidence only for PK-*or*-precognition. It seems possible to eliminate hypotheses 1 and 2 and thus get unambiguous evidence of PK (if it occurs), yet it seems extremely difficult to devise any experiment which could provide data not explicable solely in terms of non-precognitive ESP and/or PK, and which could thus

provide unambiguous evidence for precognition (if it occurs). The principles underlying this difficulty may be summed up, rather crudely, as follows.

Let us define "a precognition" as an event, *e.g.* a mental content like an image or a piece of bodily behaviour like writing a word or sentence, such that (a) a later event corresponds epistemologically with it (verifies it) and (b) its occurrence is directly causally influenced by this later event. (This definition includes what seem to be the essential features of the psychical researchers' use of this term. Taken literally, "precognition"—or "foreknowledge"—is a misleading word for describing the facts: for in the experimental situation, the "percipients" are unable to distinguish which of their guesses are hits, and in most of the spontaneous cases the earlier event is interpreted as precognitive only in retrospect, after its verification). Let us suppose that the time-gap between a so-called precognition and the later event which verifies it may be bridged by an idea or volition which persists in some one's mind. (We can probably all agree that it is meaningful to speak of "an idea or volition persisting in some one's mind", even though we would disagree about the analysis of such a statement). Now is it not always possible that, when a case of apparent precognition occurs, the earlier event (X) and the later event (Y) which verifies X are both caused by the same persistent idea or volition (Z), and that this accounts for the epistemological correspondence between X and Y? Assume, for simplicity, that Z belongs to the person (A) in whom X occurs. Then, if Y is a mental event occurring in a person other than A, Z might cause Y by way of telepathy: if Y is a physical event occurring outside the body of A, Z might cause Y by way of PK. Unless or until we can empirically determine the limits of telepathic and PK influence, how can we ever be sure that, in cases where a later event *seems* to have directly influenced an earlier event, they are not both effects of something else, in which case the earlier event is not a precognition at all?

This argument makes it appear virtually impossible to design an experiment whose results could compel one to invoke precognition. Notwithstanding this formidable difficulty, it appears to the writer that we have got some experimental data which cannot plausibly be explained without invoking precognition, namely in Whately Carington's classic experiment on the paranormal cognition of drawings. The relevant features of Carington's results were, of course, unexpected byproducts of an experiment not designed to reveal precognition. The writer has developed elsewhere<sup>1</sup> his reasons for saying that they require us to invoke precognition.

This discussion may, however, have suggested to some readers that it is idle to aim at the experimental isolation of PK and pre-

<sup>1</sup> *SPR Proc.*, op. cit.

cognition, on the grounds that an analysis of our definitions reveals PK as a species of precognition. For we defined "a precognition" by reference to two relations (one epistemological, the other causal), between a pair of events, one of which is a "human event" (mental or bodily), the other of which may be of any kind whatsoever. We suggested that "PK" should be used to refer to a causal relation between two events of more specific kinds, one of which is mental, the other being a physical event outside the body associated with that mental event. However a causal relation between a pair of such events would not be recognised, and *a fortiori* would not be classed as an instance of PK, unless these events also corresponded epistemologically.

Suppose then that there were two events, an earlier mental event and a later extra-somatic physical event, and that these were related by epistemological correspondence and by direct causal connexion. *Would there be any difference in meaning* between describing this as "an instance of PK" and describing it as "an instance of precognition"? No difference, if, as some philosophers seem to think, it is unintelligible to speak of causal connexion as having a direction. We can only distinguish the meaning of "PK" and "precognition" when applied to such a case, if we can distinguish between causal influence of the earlier on the later event, and causal influence of the later on the earlier event. The common-sense criterion of the direction of causal influence is, of course, that it holds from earlier to later events. But if this were our *only* criterion of the direction of causal influence, we could not render intelligible our definition of "precognition". We must conclude then, that if PK and precognition are to be intelligible as *independent* hypotheses (or concepts) this must be by virtue of some criterion of the direction of causal influence other than the common-sense criterion based on the temporal order of events.

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## MR. HAMPSHIRE ON FALLACIES IN MORAL PHILOSOPHY

MR. HAMPSHIRE argues in *MIND* (October, 1949) that the "typical post-Kantian approach to moral philosophy" involves several related fallacies. The cardinal fallacy is the error of supposing that moral and empirical beliefs are logically independent. I follow Mr. Hampshire with enthusiasm in his exposure of this fallacy. And it's just because I feel that what he has to say is so wholesome and important that I want to try to sort out some difficulties he raises for me in the course of saying it.

My chief trouble is with his view that a "concentration on the use of moral terms in sentences expressing a spectator's praise or blame" is a symptom of something wrong with moral philosophy. I cannot agree, firstly, that what Mr. Hampshire calls his "thesis" (p. 469) is actually constituted by this point, namely, by the view that one cannot, without danger of being incomplete or misleading, answer the question, "What are the distinguishing characteristics of sentences expressing moral praise and blame?" except as part of the question "What are the distinguishing characteristics of moral problems as they present themselves to us as practical agents?" Nor, if this were the thesis of Mr. Hampshire's paper, should I agree that it is true. On the contrary, I believe that one cannot answer the second question except as part of the first, or something like it. I shall try to show why the arguments Mr. Hampshire uses to support this particular view (which I do not regard as his fundamental thesis) appear to me unconvincing.

According to Mr. Hampshire, a moral problem is a "problem of decision". "The typical moral problem is not a spectator's problem or a problem of classifying or describing conduct, but a problem of practical choice and decision" (p. 468). ". . . the primary use of moral judgments (= decisions) . . ." (p. 469). Or a moral problem is a "practical problem" which is not merely one of ways and means: "Practical problems are . . . sub-divisible into moral and purely technical problems" (p. 479). What a moral problem is not (primarily, at least) is a problem of "allotting moral marks". A primary moral judgment is a decision; a secondary moral judgment is a criticism.

A preliminary point is this. To criticise is to do something and involves a decision. To criticise, assess, praise, blame, or allot moral marks, are as much, or as little, decisive actions as any others. Indeed, it isn't at all difficult to imagine situations in which criticism involves a practical (and non-technical) decision of the greatest importance: for example, contemporary adverse criticism by a German of Hitler's actions. It would be absurd, of course,

to suggest that this point hasn't occurred to Mr. Hampshire; he makes it himself, both in the paragraph beginning on p. 471 and in a footnote on p. 469 ("the critic of poetry . . . does not prescribe or make a practical judgment, *as does the critic of conduct or technique*" (my italics)). I find it hard to square this with what he says elsewhere on the distinction between "judgment about" or "description of" action and "choice of" action. Once you allow that a spectator's moral judgment can be a *practical* judgment, or can be "prescriptive" and not merely descriptive, you invalidate your case for saying that "we are not primarily interested in moral criticism, or even self-criticism, except in so far as it is directly or indirectly an aid to the solution of practical problems" (p. 468). For the question of what action we are to prescribe is surely as much a "practical" problem (though not the same practical problem) as the question of what action we are to perform. It seems to me that Mr. Hampshire cannot consistently hold two views which he apparently wants to hold: (1) that criticism is less important than practical choice, (2) that critical judgments are not merely descriptive, but prescriptive. For he seems to base (1) on a premise, *viz.* that criticism = description and classification (p. 468) which he rejects when asserting (2) on p. 471. The apparent inconsistency results, I think, from a misconception of the role of the moral spectator, or rather of the relation between the roles of moral spectator and moral agent.

My main point is concerned with moral disputes, such as the suicide problem adapted from Graham Greene. When my moral judgment, which according to Mr. Hampshire is a decision or choice, is disputed by someone else, it is usual to say that there is a second moral judgment. Is this second moral judgment, on the part of the disputant or adviser, "primary" or "secondary"? On the face of it, it can't be primary, because it isn't a decision for or against suicidal action. The decision to act—the decision "to be or not to be"—is mine, not someone else's; and even if the other person decided to prevent me forcibly, this decision, though relevant to the moral judgment "It is right, or best, for me to use force to prevent A's committing suicide", would certainly be irrelevant to the moral judgment "It is wrong for you to commit suicide". Is this, then, a "secondary" moral judgment, an "allotting of moral marks"? I don't think this is possible either. It seems to me quite unpleasurable to suggest that, when two people are arguing about the rightness or wrongness of suicide on the part of one of them, they are either (a) coming to opposite decisions in respect of the same action, or (b) coming to decisions to act in ways which are causally incompatible (one, say, to commit suicide and the other to prevent his doing so) or (c) uttering sentences which are on quite different moral levels, one expressing a pure decision and the other expressing a pure criticism. What one wants to say, in a case in which there is a disagreement about a "practical

conclusion", is, if one is going to use the word "decision" at all, that there are different decisions *about* a proposed action. And this is, in fact, what Mr. Hampshire does suggest on p. 474. ("Having reached this conclusion, I may of course in any particular case fail to act in accordance with it; as Aristotle points out, deciding *that x* is the best thing to do and deciding *to do x* are both distinguishable and separable.") Just as "I decided to do *x*" is compatible with "I did not do *x*", so "I decided that *x* is (or was) the best thing to do" is compatible with "I did not do *x*" or even (I am not sure if Mr. Hampshire intends this) with "I decided not to do *x*". But if Mr. Hampshire really means to assert even the milder version, he must mean that the moral decision is not the decision to act, but the decision that a certain action is right or best. And this is a decision *about* an action. But, for several reasons, it is no longer a "practical" decision (except in a sense of "practical", which Mr. Hampshire doesn't accept, in which every moral judgment is practical). (1) If a moral judgment is a solution of a practical problem, the problem is not solved by reaching a conclusion and failing to act upon it. If someone habitually fails to put his moral conclusions into practice, we do not regard him as coping successfully with his moral problems. (2) If I decide that *x* is best, and do *y* instead, my decision to do *y* (supposing that *y* is not a case of involuntary behaviour) is a practical decision, but it is not a moral judgment. (3) If I decide that *x* is best, and fail to do it, my whole "decision" cannot amount to anything more than a judgment about *x*; but a judgment about an action is what Mr. Hampshire wants to distinguish from a decision and to relegate to the class of secondary moral judgments. Consistency seems to require that a moral judgment, if it is a practical decision, must be a decision to act.

I think this would have come out more clearly in Mr. Hampshire's paper if he had developed his point about the difference between "deciding to" and "deciding that" (or "deciding about"). We can decide about an action without doing *it* (for example, if it is someone else's suicide, or our own if we are faint-hearted) but we cannot decide about it without deciding to do something else, namely to criticise or praise or recommend. Indeed, when we decide about an action and also decide to do it, we are making not one decision but two: one decision to act and another decision to approve our action. But when Mr. Hampshire says that the primary moral judgment, as expressed in a sentence of the type "*x* is the best thing to do", is a decision, he confuses these two kinds of decision. A "decision that" must involve a sentence; a "decision to" involves only a proposed action. My point is that every "decision that" is reducible to a "decision to". But can a "decision to" be "expressed"? Certainly it needn't be; and this is probably why Mr. Hampshire wants a moral conclusion to be a "decision that", even though he also wants it to be a

"practical" conclusion. But if every "decision that" contains a "decision to", and sentences "express" decisions, what is "expressed" by a sentence like "*x* is the best thing to do"? If not a "decision to", isn't it at least a "decision that"? It isn't. The point is more easily seen in the case of "secondary" moral judgments. A sentence "*X*'s action is good" does not "express" a decision to criticise *X*'s action, even though the "decision that" *X*'s action is good involves a "decision to" criticise. The sentence does not "express" even the criticism. The sentence *is* the criticism. To utter such a sentence *is* to criticise *X*'s action. To decide to utter it is to decide to criticise. The "decision to" in either case is simply the act of "making up one's mind", which is not relevant morally, but only psychologically. Even with "primary" moral judgments, it is misleading to speak of a sentence "expressing" a decision—even a "decision that"—because what is morally relevant is not the fact that a decision has been made, the fact that someone has made up his mind, but the fact that a sentence has been formulated as a conclusion of an ethical argument. What is "expressed" is not moral judgment *qua* decision but moral judgment *qua* conclusion. The "decisive" character of the judgment is of psychological interest, but it is the "conclusive" character that interests the logician and moral philosopher.

To say that a moral judgment is a decision is to say both too much and too little. Too little, because many moral judgments (all, as I believe) are not decisions to act, as the decision about suicide was apparently a decision to act. Here again Mr. Hampshire has not missed this obvious point, but he has overlooked its relevance. He speaks of people disagreeing (p. 477) as to whether capital punishment should or should not now be abolished. The moral judgments expressed by A and B are not decisions to act. They are not even decisions about action on the part of A and B faced as moral agents with the problem "Ought I to abolish capital punishment?" because abolishing capital punishment is not, like suicide, a relatively simple action within my power to perform. Nor, I think, can the position be rescued by a resort to the hypothetical: "If I were absolute dictator, should I consider it my duty to abolish capital punishment?". Because, for one thing, my being dictator would be a relevant circumstance not included in the set of circumstances in which I am asked to moralise about capital punishment. And anyway for the plain man the practical problem is simply "What ought I to do about capital punishment?" and examples of "decisions" would be writing to my M.P., or voting Labour, or issuing propaganda. But even this is not enough, because I can still argue about the rightness or wrongness of capital punishment without considering what particular actions of this sort, if any, I propose to take. To be sure, my moral judgment may be tantamount to a "dispositional" decision—a "decision"

to act in the appropriate way as opportunities for action occur. But this reduces the "action" theory of Mr. Hampshire's to the "attitude" theory which he rejects. It implies that the question "Is capital punishment wrong?" is equivalent to the question "What attitude do I maintain, or do I propose to adopt, towards capital punishment?"

On the other hand, to say that moral judgments are decisions is to say too much, because many practical decisions are not moral judgments and yet are not technical problems either. I think there is a certain amount of question-begging about the formulation of examples of moral problems in current discussions. Mr. Hampshire, for instance, asks us to "suppose one is confronted with a difficult and untrivial situation in which one is in doubt what one ought to do"; later he gives an example of such a situation and supposes that "I decide, after careful deliberation, that the right or best thing to do is to commit suicide". Now the assumption I am questioning is the assumption that you cannot, without self-contradiction, excise from the first passage in quotation marks the words "one ought", and from the second passage the words "that the right or best thing to do is"; that every "difficult" situation whose difficulty is not due to mere lack of technical skill is necessarily a moral situation (provided that the condition of "untriviality" is satisfied); that, in such conditions, "to be wondering what to do" is identical with "to be wondering what one ought to do". Now this identity is not logical. And the assumption seems to me to be an unwarranted intrusion of a psychological proposition (and a false one at that) into a logical discussion of the moral situation.

I need not stress the falsity of the psychological proposition. It will hardly be disputed that many problems do arise which are both difficult and untrivial but which are not moral problems from the point of view of the agent (Mr. Hampshire's "primary" sense). In the suicide example, I may be aware of exactly the same factual considerations as Mr. Hampshire suggests, namely that if I continue to live I cannot avoid inflicting great and indefinitely prolonged unhappiness on myself and others. But I may not worry a bit about the rightness or wrongness of suicide in those circumstances. My attitude might be describable in terms such as these. Instead of concluding "The right or best thing to do is . . .", I may conclude "The only possible thing to do is . . .". When I consider the inevitable consequences of my continued existence, I am appalled or horrified. I literally can't endure the thought of certain persons being intensely unhappy. The thought of self-extinction, or of possible eternal torment, does not horrify me. Therefore I act. But is my act not a decision, because the semi-psychological account mentions no ethical terms? Is the "therefore" in this account only a sign of a causal relation? I don't think it's as simple as that. Mr. Hampshire allows a "rational" or "logical" sense of "therefore" in which a decision not to commit suicide

"follows from" certain "arguments" which do not entail it. A similar case can be made out for a logical "therefore" in my example. I act reasonably in avoiding intense misery, even though I arrive at my "practical conclusions" without using any arguments that could be called moral.

There are decisions, then, which are not moral judgments. "What ought I to do?" is quite a different question from "What shall I do?" or "What am I to do?", though all may occur in "difficult" situations. We need a different criterion for the specifically moral problems. But I think it is worth noticing at this point why the assumption I have been criticising is so commonly made. The reason is connected with my point about disputes, and also with my point about "expressing" decisions. Calling in an adviser automatically promotes, as it were, a problem to moral status. Argument is involved and a discussion occurs. In my variant of Mr. Hampshire's example, there is no adviser and no possibility of argument. If I can't endure something, this can't be remedied by rational argument, though the adviser may be able to do or say something that will alter my feelings and power of endurance. But it is only where arguments occur that the situation becomes interesting for the moral philosopher. The moral judgment must be expressed. And, once again, what is expressed is a conclusion, not a decision. A decision without an expressed conclusion is not a moral decision. The victim of fate who finds life unendurable and quietly commits suicide is not acting as a moral agent. To say that he ought to have been is to pass judgment—to relapse into "secondary" moral judgment of the critical type.

What I feel is really important in Mr. Hampshire's treatment of moral judgments is his insistence that they are based on rational argument from factual considerations. Moral arguments do not proceed by rigorous deduction, but on the other hand they are not mere appeals to feeling, such as threats and blandishments would be. Mr. Hampshire has stressed the first distinction but has hardly, I think, mentioned the second, except where he rejects the "feeling" theory of value-judgments (p. 470). But my suggestion is that it is just here, in the distinction between the problem-situation without argument and the problem-situation involving rational argument and discussion, that we must look for the characteristic difference between the non-moral and the moral problem. It is the difference between the sentences "What am I to do?" and "What ought I to do?"; or the difference between deciding to do *x* and deciding that it is best to do *x*. I cannot agree with Mr. Hampshire that "one can only clarify the use of the principal moral . . . terms—'good', 'right', 'ought', etc.—by describing specimens of conduct to which they are applied" (p. 481). One can do this by describing situations *in* which they are applied. I have not space to do it here, but I want to mention one characteristic of such situations which is specially relevant to my argument. This is what I may

call the characteristic of generality. A moral critic's judgment about an action is not a judgment about something unique, but a judgment about a class of actions of which a particular action may be an instance. A moral agent's judgment in choosing a certain action is not, of course, a choice of a class of actions, but it is a choice of a particular action as an instance of a class of such actions. In saying that it is the best thing to do, he means that it is the best thing for any moral agent placed in similar circumstances. Mr. Hampshire gets near to saying this when he speaks of a moral decision as a choice of policy ; but " policy " suggests only a generalisation from this situation to other situations considered as possibly involving myself, not the generalisation from myself in this particular situation to every other moral agent considered as standing in a similar situation. The point is not at all new (though it has just received fresh notice in a recent article of Professor Ayer, " Analysis of Moral Judgments ", *Horizon*, Sept., 1949) but its relevance here is important. (1) A moral judgment is not a practical decision, for it refers beyond the particular practical situation, involving a judgment about the proposed action as a member of a class of similar actions by other moral agents. (2) The feature of generality which characterises the ethical situation bridges the gap between " decision " and " criticism ", thereby making the antithesis between " primary " and " secondary " moral judgments a relatively trivial one. A decision to act, if it is moral, involves a judgment about the proposed action, a favourable criticism or approval of it ; a criticism of someone else's action involves a decision, a decision to adopt or maintain a certain attitude towards that action and similar ones of its class. (3) That is why I take a view contrary to Mr. Hampshire's on the relation between the two questions " What are the distinguishing characteristics of sentences expressing moral praise or blame ? " and " What are the distinguishing characteristics of moral problems as they present themselves to us as practical agents ? ". I don't agree that the first can't be answered independently of the second. For I can and do criticise someone's action without necessarily considering what I personally would have done in the circumstances had I been moral agent instead of moral spectator. An ethical statement is general, it refers not to a particular action, in a particular set of circumstances, by a particular moral agent, but to a class of actions, in similar circumstances, by any moral agent. For the same reason, I don't see how the second question can be answered, as Mr. Hampshire says it can, independently of the first—or, rather, of the question " What are the distinguishing characteristics of sentences expressing moral judgments about actions ? ". For I cannot act as a moral agent without coming to a conclusion about my action as a member of a class of actions and being prepared to approve a similar action in similar circumstances by any other moral agent.

My last point is that Mr. Hampshire has hardly done justice

to the "attitude" theories which he rejects. He constantly links "attitude" with "feeling" in such a way as to suggest that there is little to choose, except a name, between such widely differing moral theories as those of Professor Ayer and Professor Stevenson. "It is misleading to describe (a moral judgment) as a statement about the agent's feeling or attitude" (p. 470). Critical judgments (p. 471), are, it seems, partly statements of this kind and partly "practical judgments". But to utter a sentence containing ethical terms, whether as agent or as spectator, is never (even partly) to make a statement *about* an attitude. It is to *adopt* an attitude.

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## ON NAMES OF EXPRESSIONS

ONE way of naming an expression is to put the expression itself in quotation marks. In that case, the expression is being at once mentioned and used : it is mentioned, or named, by the combination of itself with a pair of encircling quotation marks, and it is used as a part of this complex expression. While it is always necessary to distinguish between an expression and its name, it is a mistake to oppose mention and use as though mention of an expression could never include use of it.

According to the ordinary convention for use of quotation marks, we obtain the name even of an expression containing a free variable by putting it in quotation marks : thus although 'the capital of  $x$ ' is not a name, " ' $x$  is a man' " is a name of one definite expression. Since the variable ' $x$ ' is used in the name " ' $x$  is a man' ", we must say that it is here a bound variable, as it is in 'the  $x$  such that  $x$  is a human father but not anybody's child' ; and since in the function '  $x$  is a man' the variable is free, we must say that in the name " ' $x$  is a man' " the variable is bound by the added quotation marks, just as in 'for some  $x$ ,  $x$  is a man' it is bound by the added quantifier 'for some  $x$ '.

If 'P' and 'Q' are (short for) sentences, and 'A' and 'B' are the respective names of these sentences, we may have a convention by which 'A  $\triangleright$  B' is the name of the sentence (abbreviated to) 'P  $\triangleright$  Q'. Carnap would say that in 'A  $\triangleright$  B' the sign ' $\triangleright$ ' is used 'autonomously', *i.e.* as a name of the sign ' $\triangleright$ ' in 'P  $\triangleright$  Q'. This is a mistake ; both in 'A  $\triangleright$  B' and in 'P  $\triangleright$  Q', the sign ' $\triangleright$ ' is a conjunction, not a name ; in 'P  $\triangleright$  Q' it conjoins two (abbreviations for) sentences to form (an abbreviation for) a third sentence, and in 'A  $\triangleright$  B' it conjoins two names of sentences to form a third name —viz., the name of the sentence (abbreviated to) 'P  $\triangleright$  Q'. Whichever use of the sign we are speaking of, we should say " the sign ' $\triangleright$ ' ", not 'the sign  $\triangleright$ '. The ambiguity of the sign is quite harmless if the context shows which conjunctive use is intended.

Professor R. M. Martin's note on these topics (MIND, October, 1949) only shows that facility in the use of modern semantical jargon does not prevent confusion of thought. Further, Professor Martin criticises my use of the sign ' $\triangleright$ ' without referring to my explanation ; this was just careless.

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## VII.—NEW BOOKS

*Definition.* By RICHARD ROBINSON. Oxford: Clarendon Press (Geoffrey Cumberlege), 1950. Pp. viii + 207. Price 15s. net.

In this short yet very comprehensive book, Mr. Robinson seeks to bring order into the competing theories about right or good kinds of definition, and the competing classifications of definitions. He is not an obsessional rigorist, and he succeeds very well in making things tidy but not too tidy for practical use. In what follows I shall do a certain amount of untidying: but it is of a kind which Robinson has helped me to do, and which I hope he would approve of.

Definitions may be classified in respect either of purpose or of method. Thus "nominal definition" means definition whose *purpose* is to explain the meaning of a word; whereas "ostensive definition" means definition by the *method* of pointing. The purposes of definition may be divided, in the first place, into nominal and real; nominal may be divided into "word-word" and "word-thing"; word-thing definition is any process of teaching the meaning of an "elementary symbol", and may be either "lexical" or "stipulative". Lexical definition reports "the customary or dictionary meaning of a word", and stipulative definition announces someone's own choice of a meaning.

No words are lexically indefinable. If there were words whose meaning could in no circumstances be taught they would be useless for communication. Among the causes of the belief that there must be indefinable words are the fact that, in any process of giving definitions by words, there must be undefined words; and the supposition that definition must be analytic and that there are unanalysable things. All lexical definitions are imperfect, partly because of the flux of language, and partly because it is hard to do justice to all the "dimensions" of meaning. Dictionaries deal most readily with the "indicative" dimension: but there are also the "emotional", "syntactical", and "contextual" dimensions.

There is something unsatisfactory about Robinson's use of the ideas of purpose and method. If I bring about A for the sake of its effect B, we may say that the occurrence of A is part of my purpose, but we may also say that producing A is my method of compassing B; and it would be misleading to say, with no qualification or reference to B, that A is my purpose. I think people seldom seek or utter a definition for its own sake; and it would be properer to say that giving, e.g., a word-thing lexical definition is someone's *method* of achieving some ulterior purpose. The ulterior purpose may be (1) to get someone to understand a word, or to understand it better than before; (2) to add to someone's knowledge about a thing. Both these purposes may be pursued at once, and often they are. Either of these purposes might be forwarded by a word-word, a word-thing, or a thing-thing method. Consider the following examples.

- (a) "'Thermometer' means the same as 'instrument for measuring temperature'."
- (b) "The word 'thermometer' means a glass tube like this one, with a mercury column which moves up and down the graduated scale according as the surrounding matter grows warmer or colder."
- (c) "Every thermometer is a glass tube . . ." etc.

Any of these sentences might, in suitable circumstances, help to teach or clarify the meaning of the word "thermometer", and might also give someone a better grasp of the construction and use of thermometers. The typical experience of those who consult dictionaries and encyclopedias—which are not so sharply distinguished as their makers suppose—is a simultaneous passage from vague to precise understanding of a word, from vague to precise perceptual organisation—e.g. readiness to notice the exact graduations of a thermometer, instead of just a confusedly marked glass tube, and from vague to precise knowledge of the structure and properties of some class of objects: and in an obvious sense these three are one.

Robinson treats word-word definition as of little importance. He discusses at length methods of word-thing definition, and the nature of real definition. This he considers a confused category, in which are lumped together some chimerical notions, such as definition of essences, and some useful activities which ought to have names of their own, such as the analysis of "things" and the improvement of concepts. I wonder whether the current use-or-mention fashion has led Robinson to make the distinction between real and nominal unduly sharp and important. Does he perhaps go too far in saying that confusion of nominal definition with analysis of things is "the most damaging error in the theory of definition"? Had he recognised definition or analysis of concepts as *prima facie* a third species, the mingling of the kinds would have made itself more obvious. Under the heading of real definition, Robinson sometimes speaks of the analysis of a "form" or "idea", and he might, of course, hold that forms, ideas, concepts, etc., are a subclass of things: for the word "thing" covers "anything at all that can possibly be symbolised by a single word". But this is not satisfactory. For meaning a concept and meaning something other than a concept are two distinct functions which a word may exercise at the very same time: the old talk of connotation and denotation, as Robinson recognises, is not pointless. I think the man who looked up thermometers in the encyclopedia might be said to be getting a clearer grasp of the concept of a thermometer, and also learning facts about the things which are instances of the concept. The boundary between concept and facts about is vague but real. The concept has a centre and a periphery. Measuring heat is central to the concept; being made of glass is peripheral; and being used in greenhouses is not part of the concept at all.

I believe there is some confusion in Robinson's account of analysis and synthesis (ch. 5 § 3: ch. 6 § 11). Synthesis is a possible method of word-thing definition, and is also a possible form of real definition. People sometimes say "analysis" when they mean synthesis. Perhaps they do, but Robinson's examples do not seem very telling. Doing a bowling analysis or arranging statistical data are very likely mixed analytic-synthetic activities, and in such connexions, as Robinson says, "analysis" is used as the general term for analysis-synthesis. But the use of these terms in connexion with the sorting of large masses of data has little bearing on their use in connexion with the explication of a single word, thing, or idea. I think "synthesis" is tacitly used by Robinson in two senses. Sometimes " $x$  is synthetically defined" means that the definiens is of the form "what has R to  $y$ ": but sometimes it means that, not only is the definiens of that form, but having R to  $y$  is not the meaning of  $x$  (if  $x$  is a word), or is not  $x$  (if  $x$  is a thing). An example of synthesis in the first sense is the definition of " $i$ " as  $\sqrt{-1}$ ,

or of the longitude of a place in terms of the place's relation to Greenwich: in the second sense, a definition of a colour in terms of a wave length. It is true, as Robinson implies, that examples of the first kind would commonly be called analyses. The puzzling thing is to find them called syntheses. It seems to me that on this principle no concept with a complex relational structure is analysable, and we must forgo the convenient illustrations of analysis drawn from family trees.

Stipulative definitions, unlike lexical, are said to have no truth-value, being proposals, not statements. But (1) a proposal is inherently tentative, whereas the propounder of a stipulative definition means to stick to it for a certain time, whether other people like it or not. (2) A stipulative definition may truly or falsely state its propounder's intentions or habits. Being satisfied about the propounder's intentions, we pass from his definition to the tautology which it generates, and which we use in deduction. People sometimes say that tautologies are not proper statements, and by confusing the definition with the tautology they might come to say that the definition was not a proper statement. This somewhat slack reasoning, which I can hardly suppose Robinson to be guilty of, would apply equally to lexical definitions. But he doesn't say how else his assertion should be supported.

Robinson gives much excellent advice, which cannot be summarised, (1) as to effective methods of nominal definition, and (2) as to what should be kept and what jettisoned in the confused notion of real definition, in which he finds twelve distinct elements. The search for real definitions is deeply embedded in philosophic tradition and in ordinary discourse. It expresses itself typically in hopelessly vague questions of the form "what is *x*?", and the kindred question "what is the function of *x*?". Although vague questions have their uses, this vast lumping together leads to endless confusion. The name and notion of real definition should be given up except in historical connexions, and we should find new names for the useful activities which they cover. All this is admirably clear, and should be equally serviceable to philosophical and non-philosophical readers.

I should like to suggest one or two improvements in terminology. The name "stipulative definition" is not happily chosen. A stipulation is a demand laid down as a precondition of a bargain. I can see no analogue of this in what Robinson means by the word. I suggest "adoptive", which fits both the adoption of a new or circumscribed meaning for an old word and the adoption of a new word.

Robinson speaks freely of words as "signs" or "symbols". This is inconvenient, because we need to be able to say such things as (1) that people who have no common language may converse by signs, (2) that a statement which is cumbersome in words becomes manageable when written in symbols. I suppose a general term is needed of which "word", "sign", and "symbol" would be specifications. Perhaps Quine's "expression" would do.

Giving the rule according to which a word is used—usually a word which is not a name—is called the "regular" method of definition. This strongly suggests the normal, standard method, though according to Robinson this method is comparatively rare and unimportant. Why not say "rule-giving" method?

On p. 198, line 4 of last paragraph, for "not = *p*" read "not-*p*."

AUSTIN DUNCAN-JONES.

*Logic and the Basis of Ethics.* By ARTHUR N. PRIOR. Oxford: Clarendon Press. Pp. xi + 111.

THIS book is concerned primarily with the so-called 'naturalistic fallacy' in ethics, with the history of its occurrence and the refutations of it in the works of English moralists before the publication of *Principia Ethica*, and with the logical questions involved. But it contains much other matter, connected with this main topic, but of considerable independent interest. The first and last chapters are explicitly devoted to the refutation of this fallacy, the former to the logic and the latter to the history of such refutations. Chapters II, III, and IV deal with the autonomy of ethics, with special reference respectively to Cudworth, to Clarke and Reid, and to Sidgwick and his contemporaries. Chapter V, entitled *Promising as Special Creation*, is concerned with a theory as to the nature of promises which was held by Reid and has been revived by Mr. Carritt. The remaining three chapters are devoted to theories which identify or assimilate moral fittingness and unfittingness with truth or falsity. Chapter VI deals with the early history of this doctrine with special reference to Wollaston and Adam Smith; Chapter VII with a form of it which Mr. Prior ascribes to Dr. Popper; and Chapter VIII with one which he ascribes to Professor Findlay.

As regards the logic of the 'naturalistic fallacy' and of attempted refutations of it, Mr. Prior's main contentions may be summarised as follows. Unless one has some positive definition of 'natural' and 'non-natural' as applied to characteristics, the statement that goodness, e.g., is a non-natural characteristic amounts to no more than the platitude that it cannot be identified with any *non-moral* property. Suppose that a person wishes to identify goodness with, e.g., pleasantness or conduciveness to social stability. Then, provided he admits that pleasantness or conduciveness to social stability are moral characteristics, he can snap his fingers at the principle that goodness is a non-natural characteristic. Now Professor Moore has admittedly failed to provide any satisfactory positive account of what he means by 'natural' and 'non-natural' as applied to characteristics. Suppose, next, that with regard to every suggested definition of 'goodness' it had to be admitted that it is *intelligible* to suggest that a thing might be an instance of the defining properties and yet not be good. We should still not be forced to conclude that 'good' is the name of a simple quality. For another possibility would be that there is no single quality or conjunction of qualities of which 'good' is the name, but that it covers a large number of alternatives, and that whenever we try to identify it with any one of these the thought of some of the others arises and prevents us from doing so. The conclusion is that Professor Moore's arguments are useful only for dealing with *inconsistent* naturalists, who want to make the best of both worlds; but, as these are very numerous and highly respected, and as this form of inconsistency is always ready to spring up again like a weed, the arguments should always be at hand as weed killer.

In tracing the history of the refutation of inconsistent naturalism Mr. Prior shows that Moore's ablest and most cogent precursors were Price and Whateley (in criticising Paley) and Sidgwick (in criticising Bentham, Spencer, and Green). Sidgwick makes the whole point with complete clarity in his *Ethics of Green, Spencer, and Martineau*.

The main points to be noted in the three chapters on the autonomy of ethics are the following. (1) Mr. Prior thinks that the contention that

no ethical conclusion can be inferred from premises which are all non-ethical is more general than Moore's argument from 'trivialization', *i.e.* the argument that if 'good' means X then 'All good things and only such are instances of X' is a platitude. (2) The best and clearest statements of the autonomy-principle come from the *naturalists* Hutcheson and Hume. (3) We may summarise the views of the main seventeenth century protagonists in the following neat way. Take the syllogism: 'All things discoverable by reason are capable of proof; all ethical precepts are discoverable by reason; therefore all ethical precepts are capable of proof.' Cudworth and Clarke accepted both premises and therefore accepted the conclusion. Hume, Hutcheson, and Reid all denied the conclusion. The two former accepted the major premiss, combined it with the denial of the conclusion, and thus inferred the contradictory of the minor, *i.e.* 'Some (and indeed all) ethical precepts are not discoverable by reason'. The third of them, Reid, accepted the minor premiss, combined it with the denial of the conclusion, and thus inferred the contradictory of the major *i.e.* 'some things discoverable by reason are not capable of proof'. For the essence of Reid's answer to Hume in ethics is that the first principles of morals are not deductions from anything, but are self-evident; and that other moral truths are deducible from *them* and not from non-moral relationships. (4) What Mr. Prior says of Sidgwick in this connexion in Chapter IV is mainly concerned with his criticism of Kant's doctrine of the various kinds of imperative and with Sidgwick's own doctrine on this topic. Mr. Prior argues that every *general* imperative must logically be a *conditional* proposition, whose antecedent refers to *circumstances* even if not to *consequences*. The only imperatives which could logically be categorical propositions are singular ones, such as 'You ought to do so-and-so here and now'. The important distinction, as Sidgwick saw, is not between the imperatives which Kant called 'hypothetical' and those which he called 'categorical', but between both of these and mere causal statements such as: 'Unless you do *x* you will not secure *y*'.

In Chapter V, on *Promising as Special Creation*, Mr. Prior states and accepts Hume's view of the *nature* of a promise, and points out that this can consistently be held by a person who rejects Hume's utilitarian theory of the *reasons* for the obligation to keep one's promises. He summarises the former view as follows. A promise to do X is a statement of an intention to do X and of nothing further; but it is a statement made in a special way, which might be expressed by some non-indicative phrase, such as 'Let me never be trusted again if I do not do X' and it is this that gives rise to the specially urgent obligation.

Now Mr. Prior ascribes to Mr. Carritt the view that to promise to do X is to make the statement 'I hereby put myself under an obligation to do X'; that this is, from a logical point of view, in a similar position to the statement 'I am making a statement'; and that both of them are in the peculiar position that they *cannot* be false. Mr. Prior answers that the two *are* indeed alike from the logical point of view, but the likeness consists in the fact that both sentences sin against the theory of types and are therefore meaningless noises. In the case of the sentence which is alleged to be equivalent to promising to do X the type-fallacy becomes obvious in the endless regress which emerges if you try to state what is meant by 'hereby' in it.

It remains to consider the three chapters in which Mr. Prior deals with certain attempts to identify or assimilate ethical fittingness with truth and ethical unfittingness with falsehood.

In the first of these chapters Mr. Prior summarises the extreme form of this theory, put forward by Wollaston, and Hume's refutation of it. He then states Adam Smith's attempt to account for the notions (i) of fittingness and unfittingness, and (ii) of merit and demerit, in terms of the emotional and volitional reactions of a person who imagines himself to be in a similar situation to that of the agent when he acted or imagines himself to be in the position of the person affected by the act. This theory, of course, is not an attempt to assimilate fittingness or unfittingness with truth or falsehood. But Adam Smith does make an attempt to assimilate truth and falsehood to fittingness and unfittingness as analysed by him. He alleges that to accept as true the opinions of another man just *consists in* finding that one has precisely similar opinions in presence of the same facts and arguments, and is therefore precisely like approving another person's emotion or action in a given situation. Mr. Prior points out that this is a mistake. It is plainly significant for me to say that both my opinion and the opinions of those who completely agree with me may be false. But, on Adam Smith's analysis of 'fittingness', it would not be significant for me to say that B's emotions or actions may have been unfitting to the circumstances in which they occurred, if I find, on imaginatively putting myself into that situation, that I should have felt or acted as B did.

The second of these chapters contains an elaborate account, discussion, and final rejection of a theory ascribed to Dr. Popper. The theory, as stated by Mr. Prior, appears to be (i) that imperative sentences *state* something and do not merely *express* volitions or other attitudes ; (ii) that what they state is not (as with indicative sentences) propositions, but something else which may be called 'norms' ; and (iii) that norms have a property, analogous to but different from truth and falsity, which may be called 'validity or invalidity'. In the course of his discussion of this theory Mr. Prior states and examines the very ingenious analysis which Adam Smith gave, in terms of his general principles, of what may be described as 'morally approving or disapproving of another person's moral approval or disapproval'. He thinks that Dr. Popper ought, in order to be consistent, to analyse the 'validity' of a 'norm' on the same lines.

According to Mr. Prior, Professor Findlay asserted in an article (*Morality by Convention* in *MIND* for 1944) that moral sentences in the indicative merely express certain emotions in the speaker and do not state propositions ; but he combined this with the view that such sentences can be true or false, and he stated certain tests which are applicable for deciding on their truth or falsity. One point is that certain emotions, *e.g.* fear, imply certain 'claims' about their objects, *e.g.* that the object is dangerous ; and that such an emotion is counted as 'reasonable' if and only if the implied claim is true. Another point is that it is an essential part of a specifically *moral* response (a) that it is *impartial*, and (b) that the person who makes it believes (i) that no further consideration of the case would alter it, and (ii) that a similar response would be made in a similar situation by anyone who duly reflected, considered consequences, and so on. A third point is that existing uniformities in moral response have come about because each man wants his moral responses to be consistent with each other and with those of other men in similar situations, and because men deliberately adjust themselves in order to ensure such assimilation.

Mr. Prior accepts all the alleged facts, and points out that each of them has been noted and treated in some detail by either Hume or Adam

Smith or both. As regards the notion of a 'claim' made by an emotion about the nature of its object, he remarks (quoting Sidgwick) that in the case of an emotion of moral approval the claim would seem to be that the object is *morally good* or *morally right*. It would then seem to be circular to try to regard the sentence 'X is morally good (or right)' as merely an expression of an emotion of moral approval in the speaker towards X. Lastly, Mr. Prior accuses Professor Findlay of holding, or writing as if he held, that a moral utterance in the indicative which passed all his tests would be true in the literal sense in which a sentence which expresses a *judgment* and *states a proposition* can be true. He rightly remarks that this view cannot consistently be combined with the doctrine that a moral utterance in the indicative expresses *only* an emotion and *states no proposition*.

Mr. Prior's book seems to me to be excellent. It combines logical insight and analysis with most interesting historical matter. I hope that it will be widely read, and that it will lead many readers to make or to renew acquaintance with the outstanding ethical work of the eighteenth century English moralists, in particular with that of Adam Smith which has fallen into quite undeserved neglect.

C. D. BROAD.

*The Foundations of Arithmetic.* A logico-mathematical enquiry into the concept of number. By DR. G. FREGE. English Translation by J. L. AUSTIN, M.A. Oxford: Basil Blackwell, 1950. Pp. (xii + xi + 119) bis. Price 16s. net.

THIS book contains on opposite pages an exact reprint of Frege's *Grundlagen der Arithmetik* and an English translation by Mr. J. L. Austin. For ease of reference the German text has been printed with the same pagination as the original edition of 1884, and the English pages are therefore distinguished by corresponding numerals with the suffix "e". Some of Frege's references and quotations, which are not always accurate, have been corrected in the translated version, and a few additional references and notes on points of translation have been added in square brackets at the foot of the English text. Like the recent reprint of Boole's *Mathematical Analysis of Logic*, this edition was originally planned to meet the needs of Oxford undergraduates who are studying the Origins of Modern Epistemology, but it will be welcomed by all who are interested in mathematical logic and the foundations of mathematics. Mr. Austin has done his work extremely well. Wherever it is essential to reproduce the exact turn of a phrase, he has translated literally; but he has succeeded also in the much more difficult task of making Frege talk English that is at once fluent and suited to his character.

Mr. P. T. Geach has contributed the following *corrigenda*, and his suggestions have been accepted by Mr. Austin:

P. 40<sup>e</sup> l. 9, after "... property." insert sentence omitted from translation "It would be incomprehensible why we still ascribe the property expressly to a thing at all."

P. 46<sup>e</sup> l. 1, for "but that . . . concept." read "but for that very reason is only one."

P. 56<sup>e</sup> l. 25, for "general" read "indefinite."

P. 60<sup>e</sup> *ad fin.*, for "and a poorer . . . that." read "and a bad and self-contradictory one at that."

P. 103<sup>e</sup> l. 29, "one-one" (= *beiderseits eindeutig*) is too strict for

"eindeutig" (though "many-one" might be too precise in the opposite direction.

P. 119 *ad fin.*, for "have no better claim . . . than they." read "are no more real, more actual or more palpable than they." (and *wirklich* would then be better translated elsewhere by "actual" rather than by "existent", e.g. on pp. 20<sup>e</sup> and 97<sup>e</sup>). It is unfortunate that the binders have omitted Frege's name from the cover of the book, and it is to be hoped that this mistake will be corrected when more copies are prepared for sale.

Why should students of philosophy read Frege now? It is well-known that his attempt to define numbers failed because he talked of classes in a way which led to paradoxes. He admitted this himself in a postscript to the second volume of his *Grundgesetze*, published in 1903 shortly after he had received a letter from the young Bertrand Russell. And the whole project of exhibiting mathematics as a development of logic is now seen to be at least very doubtful. Perhaps arithmetic needs no logical foundations. But if it does, haven't they been provided in a more satisfactory form by Whitehead and Russell? I think the answer to this objection is threefold.

In the first place the logistic theory of mathematics will remain an interesting topic in philosophy for a long time to come. Even if it is wrong, it is as important in its way as Plato's theory of forms or the ontological argument for the existence of God. We may think it queer to ask, as Frege does, "What is the number one?", but we must make clear to ourselves just why such questions are misleading. And there is no text so useful for this purpose as Frege's *Grundgesetze*. His criticism of Kant and Mill and the psychologists and the formalists of his day is brilliant and decisive. We can see why he felt the need for something more satisfactory than the verbiage which then passed for philosophy of mathematics. But we can see also how he came to think that the numbers occurring in equations are proper names. In Russell and later writers the strictly philosophical issues are sometimes lost in technicalities or taken for settled.

Secondly, the attempt to work out a logistic theory of mathematics, whether wise or unwise in conception, has led to a very great development of logic. We owe to Frege the idea of a propositional function and the introduction of an adequate notation for multiply general propositions. As he himself said in a small paper in the *Berichte der Jenaischen Gesellschaft für Medicin und Naturwissenschaft* of 1882, written in answer to Schröder's criticism of his *Begriffsschrift*, his symbolism comes nearer to Leibniz's ideal of a *lingua characteristica* than Boole's algebraic notation or anything else that had gone before. He it was who first presented logic as a deductive system, and he knew better than most of his successors what he was about. In rigour and elegance his system is superior at many points to *Principia Mathematica*. True, there is not much of all this to be found in the *Grundlagen*, but Frege explains here the programme of his work, and there are some passages which help to an understanding of novelties in modern logic. Consider for example the following from § 53: "By properties which are asserted of a concept I naturally do not mean the characteristics which make up the concept. These latter are properties of the things which fall under the concept, not of the concept. . . . In this respect existence is analogous to number. Affirmation of existence is in fact nothing but denial of the number nought. Because existence is a property of concepts the ontological argument for the existence of God breaks down."

Thirdly, Frege's style of exposition is a model for all who think and write about abstract topics. Whether he was right or wrong, he was never lacking in subtlety, and he always said what he meant with clarity and force. For this reason, if for no other, he was justified in writing at the end of his Introduction to the *Grundlagen* : "I permit myself the hope that even philosophers, if they examine what I have written without prejudice, will find in it something of use to them." In his life-time he had little influence, but his reputation has risen steadily during the past generation, and it would be higher still if his works were more readily available. Is it too much to hope that there may yet be a complete edition? In these days of photographic reproduction it should not be prohibitively expensive to republish even the *Begriffsschrift* and the *Grundgesetze*. But if that is in fact impossible, perhaps we may at least look forward to a volume of selections which will include some of the articles he contributed to obscure periodicals.

WILLIAM KNEALE.

*Lectures on the Philosophy of Leibniz.* By the late H. W. B. JOSEPH. Oxford, at the Clarendon Press (Geoffrey Cumberlege). Pp. 188. 15s.

LEIBNIZ's metaphysical system rests on three independent sets of premisses; those derived from logic, those derived from commonsense and science, and those derived from ethics and religion. The independence of these premisses gives the system its richness; their incompatibility leads to its contradictions. Writers on Leibniz give a different picture of him according as they stress one or other of these approaches. Russell and Couturat stressed the logical premisses, Friedmann has recently put forward a plea for regarding the ethical and religious ones as paramount, and Joseph in this set of lectures confessedly follows Latta in regarding the premisses derived from science as most important. In this he expressly dissociates himself from Russell (pp. 79-80). But the difference is not so great as he suggests, for, since the premisses are independent, any complete and fairly unbiased account of Leibniz must give an account of all of them and their consequences. Joseph, like Russell, recognises this and their interpretation does not differ fundamentally. There is a difference of order and emphasis. Joseph's second chapter (the first is introductory) begins with the criticism of the Cartesian concept of matter and ends with the notion of the monad as an entity endowed with force, but the third chapter on substance is, in effect, a fresh start and, being based largely on the *Discourse on Metaphysics*, shows how Leibniz's notion of substance is based on his logical theory.

Joseph's work thus covers much the same ground as Russell's and will obviously challenge comparison with it, as being the only considerable work on Leibniz published in England since 1900. Being a set of lectures not worked up for publication it is not so complete as Russell's book, but will nevertheless in some ways form a valuable supplement to it, especially for the beginner. Thus Joseph's treatment of the scientific approach is in some ways more illuminating than Russell's in that Joseph is nearer to the difficulties of the ordinary student, especially the non-scientist, and succeeds in making clearer to him, e.g., Leibniz's reasons for rejecting the Cartesian conservation law. This chapter is, I think, the most valuable in the book.

The treatment of the logical approach to the doctrine of substance in Chapter III seems to me less good, although it has considerable value for

the beginner in showing the connexion between the Aristotelian and Leibnizian notions of substance. But it seems to me to have two considerable defects. First, Joseph's strange and apparently inaccurate terminology sometimes obscures the argument. What for instance could be meant by the statement on page 76, "Kant held the relation of divers predicates to one subject in an individual substance to be synthetic *a priori*?" I have chosen this sentence as the most obvious example of the sort of confusion involved, but it also has peculiar relevance to the argument, for corresponding to each possible interpretation of it there is a position which Joseph is possibly attributing to Leibniz. The first is that any true statement about an individual substance is analytic, the second that any true statement about the nature of substance in general is analytic. It seems to me that Joseph does not clearly distinguish between these two, and that this confusion helps him to suppose Leibniz could somehow argue from the first position to the proposition that his definition of substance as *ens completum* is a real definition. This doesn't follow from either of the above positions. Leibniz uses the first position to establish the definition; but that the definition is real follows from the view, accepted by Leibniz, that simple predicates are compatible *inter se*; and, as Joseph points out, following Russell, this cannot plausibly be presented as analytic in the Leibnizian sense.

The second defect of the chapter is that there are two points essential to the understanding of Leibniz's views on substance which Joseph either never saw or never succeeded in making plain. (1) Certain doctrines of Leibniz, notably that of creation, demand a conception of substance different from that of *ens completum*. Joseph seems to see this when he asks what Leibniz supposes to happen when God creates the world; but he does not make plain that this view is always present in Leibniz, side by side with the official view, and that it is what makes possible his view of contingency. (2) Leibniz's doctrine led to panpsychism, but Joseph does not make clear why this is so. He seems to suggest that the connexion is self-evident, when on page 66 he implies that the unity of the self and the unity which is a meeting-place of attributes are identical. But this is not obvious. A self is far from being the only thing known to common-sense which has several attributes. The answer is to be found in the first paragraph of the *Monadology*. A substance must be simple. That is, it must possess attributes in its own right, so to speak, and not as a consequence of the possession of attributes by other entities. No compound entity, and *a fortiori* no spatial objects, fulfil this requirement, because their attributes belong to them in consequence of their parts having attributes. This requirement once stated, it seemed to Leibniz that the only thing in experience which satisfies it is a mind; for a mind is not divisible in the way in which spatial objects are divisible. It is the simplicity of substance, rather than its completeness, that leads to the monads; and, since this is not brought out in the *Discourse*, Joseph's chapter seems to miss an essential point in confining itself mainly to this.

The two final chapters are shorter and less valuable. The fourth is an ingenious but somewhat over-elaborate working out of the paradoxes and contradictions which arise in the attempt to apply Leibniz's monadism in detail to the theory of perception, the fifth a somewhat perfunctory discussion of Leibniz's treatment of freedom and the problem of evil.

In spite of the foregoing criticisms, I consider this a useful addition to the scanty English literature on Leibniz.

MARTHA KNEALE.

*G. W. Leibniz.* Textes inédits d'après les manuscrits de la Bibliothèque provinciale de Hanovre. By GASTON GRUA. Paris: Presses Universitaires de France, 1948. Pp. vii + 936. 2 Vols. 600 fr. each vol.

THIS collection of texts is a valuable supplement to the existing editions. There are no revelations which will involve a modification of modern interpretations of Leibniz's doctrines, but there is much useful detail. M. Grua has concentrated on theological, moral and legal topics, grouping the papers into ten sections. Many of the papers are dated, and conjectural dates have been suggested for the rest. There are analyses made by Leibniz of books, marginal comments, letters, sketches, reports of discussions, ranging over the whole of his active life. We are in effect allowed to wander in his study, inspect the books on his table, look over his shoulder while he is writing. M. Grua's collection gives this effect better than any other collection. He is himself an admirable guide, and his footnotes relate the new papers to the vast materials already printed, correcting errors, and giving in greater fulness passages quoted by others in extracts. There are two main indexes, one of subject matter and of names, the other of the texts in other editions on which this collection throws light.

The parts of most general interest for philosophical readers are section 5, dealing with liberty and greatest good (pp. 259-508) and section 6, dealing with the soul and the world (pp. 509-561). But every section has its own attractions.

L. J. RUSSELL.

*An Index to Aristotle, in English Translation.* BY TROY WILSON ORGAN. Princeton University Press (London: Geoffrey Cumberlege), 1949. Pp. 181 27s. 6d.

Mr. ORGAN'S *Index*, which is based upon the Oxford translation of Aristotle, is intended to make it easier to obtain a complete view of his treatment of a particular topic in various treatises.

The author has, I think, dealt successfully with the various problems which inevitably arise in making an Index to a translation; and I have noticed no inaccuracy or omission or inconsistency. The contributors to the Oxford translation do not always use the same equivalent for a Greek term; and in some cases a noun and adjective which accompany each other in Greek are separated in the English vocabulary (e.g. action, practical). The author has met this difficulty by supplying cross-references and, where necessary, citing the Greek word.

The references to the pages of Bekker without the titles of the treatises may prove to be inconvenient to those who do not possess the complete Oxford translation. A simple table, showing which pages in Bekker's edition are occupied by the various treatises, might well have been given, since few of us know this by heart.

It is not clear on what principle the relative length of the article has been decided; and perhaps more space might have been given to the analysis of philosophical terms, at the expense of physiological and zoological detail. It seems strange that "perspiration" should be more fully treated than "perception."

In general, the author seems to me to have been successful in giving a condensed, but clear account of the content of a passage ; but there are some exceptions. The statement "educated man knows precision to be looked for in a study, 1094 b, 12-27" might be misleading to someone who does not already know the passage.

While I agree that the work will be valuable to readers of the English translation, I consider it an unfair blow to say of Bonitz's Index that it is "of little value to those who are interested in attaining a well-rounded view of Aristotle's analysis of a particular subject". There are few ways in which one can learn more about Aristotle than by a perusal of Bonitz's analysis of one of the chief philosophical terms, and it is absurd to pretend that he offers only a word-list.

I append some details. The use of the word "reference" seems to me too vague. Among the "references" to Plato are included both allusions and explicit citations, and it would be better to follow Bonitz in distinguishing these. In the article "demonstration", the words "subject of the *Analytica Priora*" read strangely. Probably in the passage cited, Aristotle has in prospect the argument of all four books of the *Analytics*, and therefore *Priora* should be deleted.

D. J. ALLAN.

*The Philosophy of Anaxagoras*—an attempt at reconstruction. BY FELIX M. CLEVE. King's Crown Press, Columbia University (London : Geoffrey Cumberlege), 1949. Pp. 159. 16s.

"FOR some thirty years" writes Mr Cleve, "I have been attempting to reconstruct the genuine system, in all its foundations, branches and ramifications, of one of the most gigantic of those pre-Platonic giants—the system of Anaxagoras. This fundamental condition, that all the authentic material must fit into the reconstruction, has been fulfilled. Thus, if I may venture the claim, I have finally accomplished my purpose, and this HYPOTHETICAL reconstruction, not of Anaxagoras' writing but of his philosophical system, seems to be fairly well substantiated".

It reads rather like a pronouncement by Coleridge ; but before making such a claim I think he would have considered the achievements of his predecessors more studiously than the present writer appears to have done.

A kind of typographical freakishness pervades the whole work ; and the English style, perhaps owing to a valiant attempt by the author to write in a foreign language, abounds in painful solecisms. Such mistakes are excusable in informal discussions, but I do not think that the reader of a published book should be asked to tolerate them ; and with a little care the author might have ascertained that we do not say Theophrast, Vitruv, or analogon, but Theophrastus, Vitruvius, analogy. As for "the World Ghost" (p. 120), surely philosophy is frightening enough without this visitor.

All this might be overlooked if a remarkable new glimpse of the whole system of Anaxagoras were offered ; and it is true that some of the older interpretations were unsatisfactory. But I think that what is said here had already been said with far greater reserve, explicitness and prudence by Cornford in his articles in the *Classical Quarterly* or by Bailey in *The Greek Atomists and Epicurus* ; and Mr Cleve, during his thirty years of research, might have found time to familiarise himself with these contributions. The view that "the hot" and "the cold" are regarded by Anaxagoras not as qualities but as homoiomeric things, and that this is in

his mind when he says *πάντα πάντος μοῖραν ἔχει*, is perfectly explained by Cornford ; but he does not hold—as the present writer appears to do—that these are the *only* simple substances, and that Aristotle misrepresented Anaxagoras, when he cited, flesh, bone, etc., as examples of homoiomeric substances. On the contrary, Cornford shows clearly how Anaxagoras might have arrived at his view by reflection on the process of nourishment. Further, when Mr. Cleve says of Aristotle (p. 87), “he had been the first to father upon Anaxagoras the queer teaching that such things as flesh, bone, marrow, in short, constituents of organisms, were the ultimate elements of the world”, he is in his turn misrepresenting Aristotle, who surely gives no such impression and does *not* ascribe such a view to Anaxagoras.

Several other features of the “new interpretation” can be found, if I am not mistaken, in Grote’s *Plato*, which appeared in 1867 (Vol. i, pp. 56-59).

Mr. Cleve attacks Aristotle and Theophrastus for having said that according to Anaxagoras all sense-perception is painful, and declares that his meaning was that excessive intensity of perception becomes painful. But this can hardly be right, for Anaxagoras would then have been saying what the Peripatetics themselves believed.

Mr. Cleve holds that it is wrong to regard the operation of Nous as “teleological”, and therefore unfair to criticise him, as Plato and Aristotle did, for leaving his teleology incomplete (pp. 26-27; 156). The Nous of Anaxagoras does not “set purposes”, but “knows all the mechanical possibilities lying in the elements and out of those various possible courses of a world, chooses the most beautiful and most variegated (not “the best”, which is much too hazy)”. Apparently what is meant here is that, since Anaxagoras had no notion of *creation*, the planning which he ascribes to Nous is not purposive in the full sense of that word ; and the text, which is fully preserved at this point, in fact simply says that Nous *knows* in advance all possible future combinations. Though Anaxagoras improved upon the earlier mechanistic accounts of causation, he did not rise above a kind of conscious mechanism.

But I find it hard to grasp why, because Anaxagoras lacked the notion of creation, he should have been unable to make a clear distinction between purposive and mechanical causation ; and therefore why it is improper to describe his system, or rather, the operation of Mind which he reconstructs, as “teleological”. If, with some purpose in view, I make the best of a machine which has been given to me, surely my thinking is teleological, even though I did not myself make the machine. I believe Mr. Cleve achieves his result by equating “teleological” with the untranslatable, and perhaps meaningless, German word *zwecksetzend*.

Elsewhere, discussing the question whether it is proper to describe Anaxagoras as a dualist, Mr. Cleve says that we cannot answer the question “Is Nous pure mind ?” without posing a counter-question : “Are his *moirai* pure matter ?” “There is no such gap between the purest and thinnest thing (*i.e.* Nous) and the other things as between spirit and matter. Or rather, the gap of dissimilarity is not greater than between the other things themselves, between a colour and a temperature, for instance. . . . And while Nous is extensive like ‘matter’, the material elements are never outside ‘consciousness’, because at any time they are embedded in Nous . . . and thereby known by Nous who is always present *wherever all the others are* (*Simpl. phys.* 157, 5).”

Many previous scholars, some of whom are named here, have of course said that it is a mistake to force upon Anaxagoras the opposition between

**Mind and Matter.** And where Mr Cleve goes beyond this, his view seems to be hazardous. That the elements (whether we ought to conceive these as the hot, the moist, etc., or as substances like wood and flesh) can only exist in conjunction with a knowing Mind, is far more than is said in any surviving text, and the later doxographers have missed the point badly if this was what Anaxagoras meant. At least, I do not see how it can be deduced from a text which says simply that Mind knows in advance all things that will come to be ; and such a view conflicts with the picture of Anaxagoras as an empirically-minded man of science.

D. J. ALLAN.

*Précis de Logique Mathématique.* By I. M. BOCHENSKI, O.P., Collection Synthèse 2, F. G. Kroonder, Bussum, Pays-Bas, 1949. Pp. 90.

THE author of this useful book sets himself the task of providing a text which would serve as a basis for the oral teaching of mathematical logic. He has in mind mainly those students who are interested in the non-mathematical applications of the subject. In scope and purpose this introduction is similar to Carnap's 'Abriss der Logistik' and the 'Grundzüge der theoretischen Logik' by Hilbert and Ackermann. The author uses beside the notation of 'Principia Mathematica' the notation used by Lukasiewicz and other Polish logicians ; he thus forestalls some possible errors about the role of any specific notation in symbolic logic. In the chapter on the logic of unanalysed propositions he uses the elegant and rigorous method of Lukasiewicz which is little known to philosophers outside Poland who are not specialists in logic.

The author declares that he has deliberately avoided every philosophical problem. His one philosophically controversial assertion is his statement that formalised metalogic has been rich in philosophical conclusions. Like most good books on the subject this introduction shows that mathematical logic like mathematics is philosophically neutral : it gives rise to philosophical problems but is not part of philosophy.

The book contains a chapter on the calculus of propositions, a chapter on predicates and classes, a chapter on relations and throughout very helpful remarks on the history and literature of the subject. It fulfills admirably, I believe, the purpose for which it was written.

(I have noted the following misprints : p. 8, l. 13 f.b. read : Bernays ; p. 13, l. 15 read :  $2 + 3 \times 2$  ; p. 14, l. 5 f.b. read : encore ; p. 23 No. 5. 511 read :  $pq \cdot \equiv \sim \sim p \sim q$  ; p. 35, No. 9.57 second line read :  $\vdash Y$  ; p. 40, 1.2 read : ont été et sont souvent ; p. 52, No. 15.43 read :  $\exists ! \alpha$  ; p. 61, bottom read : On ; p. 64, No. 20.11 should be followed by a verbal explanation of "D'R" and not of "C'R" ; p. 81, l. 18 f.b. read : Fre geschen.)

S. KÖRNER.

*Das Problem der logischen Antinomien.* By ERIK STENIUS, Commationes Physico-Mathematicae XIV. 11. Helsingfors, 1949.

THE author characterises his method as in Kant's sense critical and regressive. He assumes that the occurrence of antinomies, especially in mathematics, does not call for a radical reconstruction of logic and mathematics. An antinomy is in his opinion due to a clash between two lines of reasoning. On the one hand we are given an indirect proof that the concepts used in constructing the antinomy are not free from contradiction. This proof is always straightforward and there is no reason to doubt its validity.

On the other hand we are asked to believe or to infer on different grounds that the concepts in question are free from contradiction. It is here that Stenius looks for a logical mistake and for the solution of the problem. He rightly points out that to forbid the formation of certain types of concepts is in itself no solution.

An enquiry into the question of circular definitions and meaningless statements leads to a formulation of some methodological principles. In accordance with these he tackles the well-known antinomies of Russell, Grelling and others and also Skolem's paradox according to which every known axiomatic system of set-theory, if free from contradiction, can be satisfied by a denumerably infinite range of objects.

According to Stenius a statement is meaningless if, and only if, it is logically impossible to decide whether it or its contradictory is correct. In other words, an object  $a$  does not belong to the range of significance of a property  $A$ , if, and only if, it is logically impossible to decide that  $a$  possesses  $A$  or that it does not possess  $A$ . The range of significance of a concept agrees with its range of definition. It is an important feature of Stenius' doctrine that this range is not fixed but extendable by further definition; it changes as the theory involving the concept is developed. The distinction between a proper and a circular definition is not simply made in terms of 'properties' but in terms of 'properties defined with respect to objects'. In the case of a proper definition of  $A$  its defining properties are defined with respect to  $a$  before  $A$  itself is defined with respect to  $a$ . In the case of a circular definition  $A$  is defined with respect to  $a$  in terms of  $A$  with respect to  $a$ . A definition, whether circular or not, is called a "contradictory definition" if the assumption that it applies to an object is contradictory. It is important to note and seems frequently to have been overlooked that a concept can be free from contradiction within a subrange of a given range of objects and contradictory within the whole range. Stenius shows that in the construction of antinomies definitions are regarded as proper which in fact are circular and contradictory with respect to certain objects outside the original range of definition.

He holds that to avoid this fundamental mistake we must distinguish between a property as a characteristic and a property as an object and between an aggregate as one and an aggregate as many. The latter distinction is important for his sketch of a "genetic and formal" development of set-theory. An aggregate is not given as one or a whole if it is only given as many. An aggregate as a whole (Menge, set) must first be constructed from an aggregate as many (System). It follows that we cannot speak of a set of all sets since there is always a last constructed set which is not an element of any set. A system is always finite and always extendable in principle.

About half of the thesis is given up to an acute and interesting inquiry into particular antinomies conducted more or less independently of the methodological principles explained in the introduction. The last and most technical part of the thesis contains chiefly a sketch of a genetic and formal development of set-theory. It is put forward as closely related to Cantor's intention and Zermelo's system but as consistently emphasising the constructive character of Cantor's conception of a "Menge" being "a *Zusammenfassung von Gegenständen*. . . ." The central idea of this sketch is the distinction between sets and set-objects (Mengen-objekte) which represent sets but are not sets. To collect certain objects into a whole is to construct a new object *i.e.* a set-object which corresponds to

the original objects in virtue of a relation  $\beta$  which is called "a collecting relation" (*Zusammenfassungsrelation*). The development of set-theory starts with a system of individuals and set-objects. It proceeds by constructively defining a relation  $\beta$  whose range of significance or definition is extended step by step in accordance with a constructive principle (*Erzeugungsprinzip*). The nature of this principle is explained as follows. Let us say that  $\beta$  is completely defined within a domain  $D$  if, and only if, it is defined for every set-object within  $D$  with respect to every set-object or individual of  $D$ . We then define  $\beta$  completely within a certain domain  $D$  and extend this domain progressively: on the one hand for an additional set-object we define  $\beta$  only with respect to all previously incorporated set-objects, on the other hand for all previously incorporated set-objects, we define  $\beta$  only with respect to the additional set-object. Stenius defines his constructive principle more precisely. This principle corresponds to the *Aussonderungs-axiom* of Zermelo. Unlike the latter it is shown *not* to be equivalent to a denumerably infinite set of axioms. Stenius' system is thus protected against Skolem's paradox the proof of which is based on the equivalence between the *Aussonderungs-axiom* and a denumerably finite set of axioms.

All this is a necessarily inadequate synopsis of a thought-provoking thesis. Stenius's discussion of meaningless statements seems to me too brief in view of its great importance to his theory. He merely defines "*a* possesses *A*" is meaningless" by "It is logically impossible to decide whether *a* possesses *A* or whether *a* does not possess *A*." Does not the term "logically impossible" hide relevant difficulties? How shall we deal with cases where it cannot in fact be decided whether the impossibility to decide whether *a* possesses *A* is logical or not? It might further be asked whether after all the distinction between systems, sets and set-objects has not been justified only on pragmatic grounds, *i.e.* by showing that it helps to prevent the occurrence of antinomies. There still may be other ways of achieving this result which may commend themselves to mathematicians and others as being equally or more "natural".

(I have noted the following misprints: p. 20, l. 2, read: "Cauchy", p. 33, l. 22 read: "darum, dass es", p. 42, l. 22 read: "festgesetzt", p. 72, l. 1, read: "Mengenobjekte", p. 85, l. 2 f.b. read: "aktual").

S. KÖRNER.

*Modern Science and its Philosophy.* By PHILIP FRANK. Cambridge: Harvard University Press, 1949. Pp. 324.

ARE there two kinds of truths in the universe, a scientific and a philosophical? This is the central question in Professor Frank's selection of sixteen of his essays, 1907-1947. His major aim is to bridge the gaps; his major method and orientation is "logical empiricism". I will confine myself here to indicating the direction of his thought by abstracting what seem to me the basic themes in his book, and suggesting some lines of criticism.

1. *That the law of causality involves circular reasoning.* Frank's position is that the various qualities which we ascribe as properties to the state of a system, if they are not qualities which are empirically verifiable, confuse rather than clarify our knowledge of the world. Thus, "the law of causality is only the establishment of a terminology" whose sole purpose is to confirm to our "common sense" that "causality" is really an active

agent. It is the old epistemological battle of *vérité du fait* versus *vérité de la raison*, and Frank sides with the former however much the latter makes the world *seem* more intelligible to us.

2. *That truth is the invention of propositions which can be verified by the senses and is not in the world for all time waiting merely to be discovered.* Following Mach, truth for Frank is regarded as the correspondence of a logically coherent system of symbols with large classes of phenomena empirically verifiable by intersubjective agreement. However, like Mach, he is not opposed to the use of "auxiliary concepts" in science, *e.g.*, force, gravity, atom, for they are but a shorthand way of conceiving what is demanded by the logical symbolic structure of our theoretical edifices. So long as these auxiliary concepts are not reified and assumed to be "out there" in essence, science benefits from them.

3. *That anti-metaphysicalism (or logical empiricism) can be the basis for a unification of the sciences.* If every scientific proposition is a statement about relations of sense perceptions, then it follows that in order to unify the sciences we need to co-relate our scientific statements of the various fields. This can be accomplished by the elimination of the *je ne sais quoi* qualities of metaphysicalism. For metaphysics divides men and theories; logical empiricism unites them.

4. *That Science knows that it does not know but denies that it can never know.* In total opposition to such statements as *ignoramus ignorabimus*, Frank shows by numerous examples how the "tough-minded" attitude has, by the stubborn poking and prodding of nature, forced her to yield her "secrets" and successively demolished causal "spooks" postulated by the "tender-minded" metaphysicians and scientists.

5. *That mathematical formulations in science and recent advances in quantum mechanics do not imply "idealism", "spiritualism", "free-will", or "vitalism" nor do they imply "materialism" or "mechanism" as the ruling principle of the universe.* Logical empiricism is absolute on one matter only, *viz.*, relativity, *i.e.*, it is absolutely relative. It seeks to understand and "explain" the world in terms of propositions which agree with observed phenomena—regardless of whether these propositions stem from "idealistic" or "mechanistic" schemata.

6. *That metaphysics and common sense are merely the abandoned science of the past.* For Frank, the recurring disagreements between science and philosophy stem from the notion that there are two truths: (a) that based on the observation of phenomena—"saving the appearances" in Plato's language; (b) that based on the notion that there is something beyond the observations—metaphysics. The history of science has been the struggle between those who wish to emancipate (a) from (b) and those who wish to subordinate (a) to (b). The more the emancipation of (a) from (b) the more the progress.

The following are a few questions which the reviewer would like to see better answered.

1. Logical empiricism indicates the "howness" of the world, but not the "whyness". The latter, it is asserted, is meaningless. *Why?* Can it be that there are answers to both questions, only the "howers" fear to tread in the regions of the more *tough-minded* "whyers"?

2. Truth, for Frank, is the agreement ("unique correspondence") of a conceptual system with a body of verifiable phenomena. Question: How do we know when we have an agreement? What, for example, is an agreement of the theory of evolution with verifiable facts? If this cannot be shown, is such a theory, therefore, automatically meaningless?

3. Frank asserts that all scientific propositions must have a method for subjecting themselves to *experimenta crucis*. Question: How do we know when our method, if we have one, has *relevance* to our propositions?

The reviewer would also like to make two suggestions. 1. Frank does not adequately distinguish between philosophy and metaphysics. This distinction (if any) might make for greater clarity of presentation. 2. Frank speaks of "science" but one feels he is continually thinking only about physics. One would like to see more references to the other sciences in a book which calls itself "Modern Science. . . ."

In fine, we may say that Frank is a philosopher and scientist of enlightenment in the anti-metaphysical tradition of Hume, Voltaire, Comte, Mach, Poincaré, and Einstein. He seeks "a complete disintegration of traditional philosophy". This is the basis for a unity among the sciences. The crucial problem is: Can an adequate de-mentalized psychology ever be achieved? Or must there remain a definite schism between the sciences, mental and non-mental? Until such time that a solution is found, an attitude of mutual tolerance, even mutual aid, in the respective camps can be the only attitude which will benefit the progress of science and its philosophy.

It is an altogether powerful book. None who reads it can remain unmoved.

PERCY BLACK.

*Matter, Mind and Meaning.* By WHATELY CARINGTON. Methuen & Co. Ltd. Pp. xx + 257. 12s. 6d.

MR. WHATELY CARINGTON was convinced that the investigations of psychic researchers had brought to light a considerable body of observed facts which were odd in that, though as hard, observable and intersubjective as the scientist could wish, they seemed not to fit at all easily into the conventional scientific world-picture. He did not by any means regard these facts as lying outside the province of science, but he did not think that the time was yet ripe for the scientific investigation of the relation of mind to matter which such phenomena as psychokinesis called for. Carington considered that the language and linguistically conditioned presuppositions of philosophers, scientists and ordinary men alike embodied and implied undesirable metaphysical "nonsense" which effectively prevented a detached scientific investigation of psychic phenomena; his aim in this book is to clear the decks for scientific action by sweeping away the metaphysical encumbrances and providing an aseptic language which the future mathematical psychologist would find to be a tool adapted to his purpose.

This is an admirable aim, and Carington pursues it in a lively and original manner. But his philosophical technique is perhaps not adequate for the accomplishment of his programme. The neutral monist metaphysic which he advocates is put forward not merely as a linguistic device but as a truth about the world established by *a priori* arguments the joint premises of which are the incompatible verification theory of meaning of the positivists and the Ogden and Richards theory of meaning. Thus he goes further than he needs, and in so doing he sticks out his neck under many philosophical axes. If he had been content to argue in greater detail for a neutral terminology which did not manifestly presuppose all kinds of things which the psychic researcher must question he would have been on safer ground; though, even so, a description of the world in

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The terms of 'cognita' (Hume's impressions and ideas of both inner and outer sense) is perhaps neither so practicable nor philosophically respectable as Carington supposes. From the philosopher's point of view the merit of the book is that it tries to set the right kind of question, not that it answers it.

The author unfortunately died before the book was complete. It was prepared for publication by Professor Price, who also contributes a judicious preface. No doubt if the author had lived some imperfections outside the jurisdiction of an editor would have been removed. But even as it stands, it is brightly written, asks the right questions, and will stir the philosophical wits of many who are not the normal audience of professional philosophers.

J. O. URMSON.

*The Structure of the Universe: An Introduction to Cosmology.* By G. J. WHITROW. (Hutchinson's University Library). Pp. 171. Price 7s. 6d.

PHYSICAL cosmology is a subject of peculiar obscurity. On the one hand, the amount of experimental and observational data at the theorist's disposal is necessarily small; on the other hand, it is more than usually difficult to see what bearing these data have on the choice between the different theories proposed. Furthermore, around these unavoidable obstacles to understanding there drifts the smoke of a battle between theoretical physicists, whose respective positions seem to each himself simple common-sense, but to the others sheer metaphysical dogma. The outsider who reads popular expositions by an interested party, such as Sir Arthur Eddington, may be excused for thinking that the searchlight which he plays upon the subject throws more into darkness than it illuminates.

Dr. Whitrow's book is a refreshing change. Although he is not a neutral in this dispute (being in fact a pupil and collaborator of Professor E. A. Milne), he has done a great deal towards disentangling the issues involved, and presenting them in a manner which the tyro can appreciate. It is idle to hope that this can be done completely, and Dr. Whitrow does not seriously pretend to do so (it is a little late in the day, for instance, after all that has gone before, for him to stop and explain on p. 102 that 'momentum' is the 'product of mass and velocity'). Still, he is genuinely aware that there are special difficulties to be faced, difficulties which can in one sense of a hard-used word be called 'philosophical', and he attacks them with a will.

The most valuable parts of his exposition deal with the astronomical background and evidence (Chaps. I and II) and with the theorists' 'world-models' (Chaps. V and VI). His historical treatment of the philosophical theories of Space and Time, and his concluding chapter on 'Cosmology and the *A Priori*' are rather disappointing by comparison. In his discussion of 'world-models', however, the weaknesses as well as the strengths of his approach are clearly displayed. He rightly recognises on occasion the importance of understanding how it is that we can represent the astronomical history of the universe in a number of different ways—using, for example, as Milne does, two alternative 'time-scales', on which 'time' respectively can and cannot be spoken of as having a 'beginning'. On other occasions, however, he forgets what he there seems to have recognised, namely, that we have not just got to *measure* the age of

the universe-as-a-whole, but that we have first to give such phrases as 'the age of the universe' a *meaning*. A great merit of Milne's work is that it reminds us that how we do this depends on how we choose our theoretical 'time-scale' (cf. the Absolute Zero of Temperature), and that there may be no absolute superiority of one scale over others—indeed, perhaps one should say, *can* be none. The term 'world-model' is helpful as long as it reminds us of this element of choice in physical theories : it is harmful, however, if it encourages us to think that the theoretical schema bears the same relation to the phenomena for whose representation it is used as an orrery does to the solar system, or the driving-wheel of one of Mr. Bassett-Lowke's products to that of the full-sized ' Flying Scotsman '. It is just the differences between these things which is the chief source of difficulty, and which gives the theoretical work of Milne and Eddington much of its point.

It would be unreasonable to complain because Dr. Whitrow is not clearer in his treatment of the subject, for any such gain in clarity would itself constitute a major advance in cosmology. Instead we must be grateful for a stimulating survey of the subject in its present state.

STEPHEN TOULMIN.

*Philosophical Studies. Essays in memory of L. SUSAN STEBBING.* Published for the Aristotelian Society by George Allen and Unwin Ltd., 1948. Pp. v + 148. 15s.

L. SUSAN STEBBING, Professor of Philosophy in the University of London from 1933, died, lamentably early, in September 1943. The war-time conditions under which it was produced doubtless account for the delayed appearance of this memorial volume. The book contains ten essays by friends, including former pupils, of Professor Stebbing on philosophical subjects allied to some of those in which she was interested. There is nothing, however, directly on the problem of perception, on the relations between philosophy and science, nor on aesthetics. Nevertheless, one is grateful to the Aristotelian Society for this tribute to a distinguished member and past President. Welcome, too, is the reprint of Mr. John Wisdom's appreciative memorial notice from MIND 1943 and the excellent photograph with which the book is prefaced. The book also contains a bibliography of Miss Stebbing's published writings.

Three essays are on ethics, five on logic, epistemology and philosophical method, one on psychology, and one on history. Professor H. B. Acton on *Moral Ends and Means* maintains that a moral rule may be justifiably violated to achieve a further end only (1) when the end is clearly specified, (2) is good, (3) can be realised within reasonable time, (4) does not require frequent violation of moral rules. From Professor Acton's discussion the question "Should bad means be used to achieve good ends?" reduces to that of when one moral rule should take precedence of another but I doubt if this gets to the heart of the worry about means and ends. Mr. A. E. Duncan-Jones in *The Concert Ticket* distinguishes three types of 'moralities' within which people act : (1) Limiting or conventional, (2) Purposive, (3) Comprehensive. He tries to show by a particular example how the moral framework may 'stretch' during prolonged moral reflexion. The example might have been curtailed with advantage to allow of some discussion of the relation between 'moralities' and their justification. The late Professor J. Laird in *Reflections occasioned by Ideals and Illusions* discusses again the work in which the times evoked from Miss Stebbing

something like a moral and political 'credo'. Though mainly sympathetic Professor Laird finds mild fault with her over-intellectual insistence on the clear definition of moral ideals and claims, moreover, that she fails to observe her own rule in some of her more positive assertions *e.g.* those about 'democracy'.

By far the most important philosophical contribution in the volume is the essay by Professor Max Black on *Logic and Semantics*. This is a clear, critical, and authoritative account of the contribution of the new discipline of Semiotic to the definition of logic. Professor Black's study is illustrated entirely from the work of Carnap, as the chief representative of the school and its most prolific writer. He concentrates attention on the various accounts by Carnap of logical implication. Both the earlier 'syntactical' and later 'semantical' versions are subjected to vigorous analysis and criticism. Prof. Black's conclusion is that the new methods, whatever their technical value, have provided no answer to the philosophical problem of logic. Moreover, it is highly unlikely that they will ever be able to do so on the peculiar interpretations of both 'language' and 'logic' which they assume. Professor C. A. Mace in *The Logic of Elucidation* discusses "the philosopher's proneness to be misled and to mislead" by any utterance in a philosophical context and calls for a logic of elucidation in addition to that of proof and refutation to mitigate dialectical cross purposes. Dr. A. C. Ewing in *Philosophical Analysis* compares two views of the relation of a proposition to that which expresses its analysis. (1) That of Moore in which the expression of the *analysandum* must be synonymous with that of the *analysans* and (2) Professor Stebbing's view of 'directional analysis' in which the *analysans* expresses a set of basic facts implied by the *analysandum*, and which are its truth grounds, though the two may not be identical in meaning. Dr. Ewing gives modified support to Miss Stebbing's view.

Professor Stebbing was for a time much influenced by Whitehead and it is, therefore, fitting that the volume includes an essay by Dr. Ruth L. Saw on *The Grounds of Induction in Whitehead's Philosophy of Nature*. Dr. Saw gives a clear and critical exposition of Whitehead's attempt to answer Hume by showing that a single instance of the perceived co-existence or succession of events exhibits necessary connexion and so affords a reliable basis for induction. Professor L. J. Russell in *Epistemology and the Ego-centric Predicament* challenges Lord Russell's assertion that scientific procedure makes doubtful assumptions, *e.g.* about the existence of an external world and other investigators, which philosophy must justify. This springs from unreal doubting and an obsession about the superior certainty of statements about personal experience. Professor Russell maintains that private statements are not more certain than public and science needs no outside help in establishing its conclusions.

Finally, the late Professor Beatrice Edgell in *The Way of Behaviour* examines a recent attempt by Professor. Wolters to give a behaviouristic account of conceptual thought and Dr. H. D. Oakeley asks *Is there Reason in History?*

I have noticed misprints on the following pages, 78, l. 4; 82, l. 1; 108, l. 18 'is' omitted; 111, l. 26; 132, l. 34; 143, l. 15. There is also an important omission from the bibliography, *viz.* the essay "Some Ambiguities in Discussions concerning Time" contributed by Professor Stebbing to *Philosophy and History*, a volume of essays presented to Ernst Cassirer and published in 1936.

MARGARET MACDONALD.

*Philosophy of Mathematics and Natural Science.* By HERMANN WEYL.  
 Princeton University Press (London : Geoffrey Cumberlege), 1949.  
 Pp. x + 311. \$5.00 ; 40s.

THE first 218 pages are a revised and augmented translation of Professor Weyl's "Philosophie der Mathematik und Naturwissenschaft", which originally appeared in Oldenbourg's *Handbuch der Philosophie* in 1927. Professor Weyl says in the preface : "Writing it, I was bound by the general plan of the *Handbuch*, as formulated in broad outlines by the editors, that laid equal stress on both the systematic and historical aspects of philosophy. I was also bound, though less consciously, by the German literary and philosophical tradition in which I had grown up." This is a pity, for the train of thought is continually interrupted and obscured by references to the confused epistemological utterances of philosophers. For example, would not the extract from Fichte on p. 130, "Translucent penetrable space pervious to sight and thrust, that purest image of my knowledge, is not seen but intuited, and in it my seeing itself is intuited. The light is not without but within me, and I myself am the light", have been better left in the obscurity of *Werke*, ed. Medicus ? The thoughts of so eminent a mathematician as Professor Weyl cannot fail to be welcome to philosophers, but they would have been more welcome if we could have had them "neat". (We do get them in a less diluted form in the six appendices specially written for the English edition, pp. 218-301.)

The first and original part of the book is itself divided into two parts. Part I is on Mathematics, and it is especially interesting in that it is written very largely from the intuitionist standpoint. It is unfortunate that the intuitionist outlook, which has so much to teach us, is presented in the quasi-psychological idiom of epistemology, but those readers who can separate the logical grain from the psychological chaff will surely acquire a far deeper understanding of the foundations of mathematics. Part II on Natural Science, with chapters on "Space and Time", "Methodology", and "The Physical Picture of the World", seems to be less valuable ; the most interesting remarks are those on pp. 151-164 on the formation of theories and the simplicity of laws. The discussion of probability on pp. 195-197 seems to be vitiated by a confusion between possibility and probability. Thus on p. 195 we get the phrase "equally possible". Can something be more or less possible ? I should suspect nothing more than a too literal translation of "gleich-möglich" were it not for the fact that on the following page Bernoulli's Theorem is regarded as "the bridge from the subjective to the objective conception of probability". In this part there are also certain notable category mistakes. On p. 116 we have "my consciousness, crawling upward along the life line of my body". (How fast does it crawl up ?) Similarly on p. 194 we are said to "travel along the world line of our body" and there is a reference to "the one-way direction of the flow of time". (Compare also the curious question in Appendix C, p. 264, as to "why light is emitted only 'towards the future'".) On p. 87 we find that "The metric field makes itself felt through the physical effects which it has upon rigid bodies, upon light rays, and all events in nature" ; this may be all right as a mere figure of speech but it does not bear thinking about.

To pass to the new part of the book, Appendix A on "The Structure of Mathematics" is extremely interesting. It contains a discussion of Gödel's Theorem, which Weyl compares with the paradox "This statement is false". He puts his finger on the trouble with the latter, saying that

"the paradox of the pseudomenos depends on the demonstrative words 'this', 'I', 'now', by which the meaning of the sentence explodes" (p. 229). In the case of Gödel's proposition Weyl holds that "no explosion takes place". Appendix B is concerned with combinatorial analysis and some metaphysical applications of the technique. One curious result of the importance that Weyl seems to attach to metaphysical utterances is that the Pauli exclusion principle is referred to as "the Leibniz-Pauli exclusion principle", on the ground, apparently, that the principle comes to saying that electrons satisfy Leibniz's *principium identitatis indiscernibilium*; surely Pauli's principle was asserted on account of certain spectroscopic facts, and we can conceive that the facts might have been such that we could do without it, whereas Leibniz's principle was supposed to be an *a priori* truth, and so there cannot be the least connexion between the two things. Appendices C, D, and E are on Quantum Physics and Causality, Chemical Valence, and Physics and Biology respectively, while F is on questions of cosmology and evolution.

The title of *Philosophy of Mathematics and Natural Science* testifies to a general danger. The problems of the logic of physics (however much mathematics it may contain) are so different from those of the logic of pure mathematics and are yet so easily thought to be similar to them, that perhaps the two things ought never to be discussed within the covers of the same book. Professor Weyl is so eminent a mathematician that many parts of his book cannot fail to be highly instructive and stimulating, but taking it as a whole I feel that there is an almost exclusive emphasis on the most abstract and theoretical aspects of physics and that many readers may be led to overlook the way in which physical theory connects up with experiment and common language.

J. J. C. SMART.

*A Treatise on Language.* By ALEXANDER BRYAN JOHNSON. Edited, with a critical essay on his philosophy of language, by DAVID RYNIN. Berkeley: University of California Press, 1947. Pp. ix + 443. \$5.00.

"PERHAPS if ideas and words were distinctly weighed and duly considered, they would afford us another sort of logic and critic than what we have hitherto been acquainted with." It was this possibility of a new 'sort of logic and critic', a possibility which Locke had begun to suspect at the end of the *Essay Concerning Human Understanding*, that Alexander Bryan Johnson set out to realise in his lectures on the *Philosophy of Human Knowledge*, a work which in later and expanded versions was significantly retitled, *A Treatise on Language*.

Johnson was a highly successful, self-made, American banker, who lived in the first half of the nineteenth century. These lectures—originally delivered to the Utica Lyceum in 1825—were the product of studies made solely in his spare time. Nevertheless Johnson's ideas have the most extraordinarily modern ring, though they are expressed in a pungent and picturesque style which is all his own. Take for example a passage from his final manifesto (p. 300). "Our misapprehension of the nature of language has occasioned a greater waste of time, and effort, and genius, than all the other mistakes and delusions with which humanity has been afflicted. . . . The misapprehension exists still in unmitigated virulence; and though metaphysics, a rank branch of the error, is fallen into

disrepute, it is abandoned like a mine which will not repay the expense of working, rather than like a process of mining which we have discovered to be constitutionally incapable of producing gold." Or again (p. 77), "The word gravity names many interesting and important phenomena; but if, in addition to these, we look for gravity itself, we act as ignorantly as the child at the opera, who, after listening with impatience to the musick, singing and dancing, said, 'I am tired of these; I want the opera'." Often Johnson makes his points in the form of criticisms of his predecessors. For example, on Hume's complaint that, "our senses inform us of the colour weight and consistence of bread; but neither sense nor reason can inform us of the qualities which fit it for the nourishment and support of the human body". Johnson comments (p. 278), "So long, however, as the proposition of Hume has any signification, it is untrue. Our senses can discover every phenomenon which is exhibited by bread, therefore they can discover the qualities which fit it for nourishment. To use the word quality insensibly makes the discovery difficult indeed; for we prosecute it under this disadvantage, that nothing which we discover can be the object sought. The very circumstance that our senses discover it being conclusive against it; for the conditions of our search are that 'neither sense nor reason can inform us'." Good things such as those we have already quoted are to be found in abundance throughout the book. But perhaps the richest lectures are Nos. IV and V (from which the remarks about gravity are taken, and which are epitomised in the epigram, "The search after the unit is the delusion"), No. VI ("Words can be divested of signification, and still formed into propositions which are not obviously unmeaning") Nos. XIX and XX (about questions and pseudo-questions) No. XXII ("Inquiries after the definition of words we mistake for an investigation of nature") and No. XXVI ("We mistake the unintelligibility of a word or a proposition for a mystery of nature").

The quotations should be sufficient to show that this is a remarkable book. The text printed in this edition is a conflation of the three different versions published by Johnson. Dr. Rynin has added a 'Preface' explaining his method of conflation, an 'Introduction', giving a short biography of Johnson and a list of his publications, a 'Critical Essay on Johnson's Philosophy of Language', an appendix providing an 'Identification of passages quoted or referred to by Johnson', and, finally, an Index to the whole volume. The Critical Essay is divided into two parts: in the first Dr. Rynin tries to lay down, "the minimum requirements, of any adequate philosophy of language"; in the second he attempts to estimate Johnson's achievements and deficiencies by this yardstick. The deficiencies are the usual deficiencies of enthusiastic and unsophisticated empiricists: Johnson (like Hobbes and many more recent writers) tended strongly to treat all words as names; he tried to analyse all empirical sentences solely in terms of what their users had already experienced or were already experiencing, and not, more plausibly, in terms of actual and possible experience; he got himself into difficulties in trying to reconcile the truth and significance of theological statements with his general principles; and so on.

This book should be in every philosophical library, not merely for its historical interest, though that is certainly great, but also because of its considerable pedagogic value. Dr. Rynin has done a good service in rescuing Johnson from unmerited oblivion.

*The Life of Reason.* By W. G. DE BURGH. London: Macdonald and Evans, 1949. Pp. xxiii + 219.

THIS posthumous work by the late Professor de Burgh has been edited by Mrs. de Burgh with the co-operation of a number of distinguished scholars, including the late Professor A. E. Taylor and Dr. Clement Webb, the latter of whom contributes a short Foreword. De Burgh was still engaged on it at the time of his death, and it was therefore unrevised by him and unfinished. The general trend of the argument, however, is complete, and its main features will be familiar to those acquainted with his earlier books. His aim is to protest against the limitation of the function of reason to the processes of discursive reasoning and to argue instead for 'an enlarged view of reason—a view that will sanction the inclusion of intuitive thinking, aesthetic and scientific imagination, the higher levels of emotion, and moral and religious faith, within the scope of the intellectual life'. The function of reason is synthesis, yielding knowledge. Human reason, then, is exhibited on various levels of human activity, and leads to corresponding forms of knowledge approximating in varying degrees to what de Burgh takes to be the ideal, namely intellectual intuition. He examines the various forms of speculative reason (science, history, art, philosophy and religion) and of practical reason (law, morality, religion) and, by means of certain criteria which he provides, grades them on a scale according as they approximate more or less closely to the ideal of intuitive understanding. All of these activities, he holds, are reasonable in the wide sense of the word, and all of them yield knowledge, religion occupying the highest place on the scale. Finally there is a long chapter on 'The Problem of Immanence and Transcendence' in which certain views of Alexander are discussed, with particular reference to the Christian doctrines of the Incarnation and Creation, and in which, incidentally (p. 190 f.) the argument from Analogy in its traditional form appears to be accepted more easily than in the earlier book *From Morality to Religion* (p. 170 f.).

For the book as a whole, perhaps it is sufficient to say two things. First, it is welcome in its insight, scholarliness, and high-minded sincerity, as a further reminder of the character of de Burgh himself. Second, it is written in the idiom and modes of thought common to de Burgh and the other well-known figures of his generation. 'The Logical Positivists' appear not infrequently, but their appearance is a signal that there is mischief afoot; and the logic accepted without question is the logic of Aristotle and Aquinas. This is what one would expect, and is no criticism of the present book. But it would be a pity if others with interests and insights like those of de Burgh should, in seeking to communicate them to their contemporaries, follow him in using the old modes of thought and the old idioms unquestioningly, and remain impervious to modern influences in philosophy.

R. C. CROSS.

*Kierkegaard's Philosophy of Religion.* By REIDAR THOMTE. Princeton University Press, 1948. London: Geoffrey Cumberlege, 1949. Pp. viii + 228. Price \$3.50, 18s. net. (pre-devaluation).

THIS book aims at giving a sympathetic and straightforward account of Kierkegaard's thought, and as far as I (who am anything but a Kierkegaard specialist) can see, it does it very well. It is not always easy reading—but perhaps no book on Kierkegaard ought to be. It is, however, well

arranged, and gives the impression of a coherent whole. There are useful summaries of Kierkegaard's leading ideas on various issues ; and throughout the author succeeds in presenting both Kierkegaard's matter and his style, which is of great importance. There is also a valuable discussion of Kierkegaard's method of communicating his ideas, under the heading of "Socratic midwifery". References are copious and detailed, and there is a gratifying supply of quotations, including many from Kierkegaard's untranslated papers.

It is impossible here to give anything like an adequate account of the contents of the book, but I shall refer to two points which the author makes clearly and well. Even those who know hardly anything about Kierkegaard have usually heard of his doctrine of the "Three Stages"—the Aesthetic, the Ethical, and the Religious. Mr. Thomte points out (a) that within the Religious Stage Kierkegaard distinguishes sharply between two types of religion, the religion of Immanence and Christianity. According to the religion of Immanence, "truth is subjectivity", human nature is fundamentally good, and God is "immanent in the human personality"; but according to Christianity, "subjectivity is the untruth", and "every Christian is such only by being nailed to the paradox of having based his eternal happiness upon something historical". Nevertheless, according to Kierkegaard, Christianity *includes* all that is of value in the religion of Immanence, and one can only become a Christian *via* this other type of religion ; and this is an important difference between Kierkegaard and many modern theologians who have been largely influenced by him. Furthermore (b) the Kierkegaardian "Either/Or" (the absolute self-committing choice) holds, according to Mr. Thomte, not between each pair of Stages, but only between the Aesthetic on the one hand and the others taken together on the other ; there is a *continuity* between the Ethical Stage, the religion of Immanence and Christianity, but a sharp breach between the Aesthetic and these. This, Mr. Thomte admits, runs counter to a common interpretation of Kierkegaard, but he seems amply to justify his view by reference to the texts.

Certain points, however, are still obscure, to me at least. I am not yet at all clear what Kierkegaard means by "pathos" or by "repetition" ; nor have I really grasped his distinction between "guilt" and "sin"—and as the difference between the religion of Immanence and Christianity appears to turn largely on this, that is a somewhat serious matter. There is room, I think, for a re-writing of the passages which deal with these matters in (let us say) words of two syllables, especially in a book which is intended as an introduction.

Mr. Thomte does not attempt a philosophical critique of Kierkegaard's ideas, and for the right reason—that for the most part such a critique would be impossible, in that to attempt it would be to be guilty of a misunderstanding : to such an "existential" writer only an "existential" response is appropriate. I certainly do not mean that Kierkegaard is above criticism or that we should not be prepared to disagree with him ; but on the whole it is true that he does not so much enunciate propositions with which one is invited to agree or disagree as issue a challenge which one either accepts or does not. But the trouble is this, that Kierkegaard was, to put it mildly, a very unusual kind of person, and the eruptive melancholia of his temperament colours his writings through and through ; so that although his forceful character and his undeniably great literary gifts made him eminently capable of writing in this challenging way, the idiosyncrasies of his temperament unfitted him, it seems to me, for the

presentation of Christianity in a way which would have the universality of application which he undoubtedly desired. One instance of this is indicated by one of the few critical comments Mr. Thomte allows himself. What possible appeal, we may ask, could a Kierkegaardian Christianity have for children? According to Mr. Thomte, Kierkegaard holds that "the Christian experience is too violent and severe to fit the life of a child." (p. 208.) It is very far from my intention to sentimentalise or to write a sermon, but this point seems to me a crucial one when what is under discussion is Christianity (*vide* the Gospels, *passim*). I do not wish to disparage the greatness of Kierkegaard, or to deny that he stressed, more forcefully perhaps than anyone else in modern times, certain vital elements in the Christian religion to which many Christians were (and are) blind; yet I suspect that it is only those who temperamentally resemble him who will be able to give to his ideas, in the form in which he presents them, that full "existential" acceptance in which alone he was interested.

Mr. Thomte is a native of Norway, but, apart from a very occasional word or phrase which does not ring quite true, his book is written in the most excellent English. I wish, however, that he had not so constantly used the rather disreputable word "religiosity", instead of the alternative "religiousness" which, though barbaric, is at least neutral (in fact, very often the simple word "religion" would have done); but I gather that the original culprit here is the late Professor David Swenson rather than Mr. Thomte himself. I have noticed only one misprint: on p. 121, line 13, "There" should read "These". The production of the book is excellent.

GEORGE E. HUGHES.

*An Introduction to Aesthetics.* By E. F. CARRITT. Hutchinson's University Library. 7s. 6d.

THE contents of this book have to some extent appeared in earlier works by Mr. Carritt, but here his views are presented more fully, with an entertaining wealth of example and illustrative quotation; and besides this, there is a useful appendix, of statements from a variety of thinkers and creative artists, all somehow reminiscent of Mr. Carritt's account of beauty.

But this account, on the whole, tends to leave the reader uncertain. The author explicitly states (pp. 20-21) that the *meaning* of "beautiful" cannot be given; his concern is to identify that "other common quality" that is its universal concomitant. But consider the following three quotations:

- (i) "the most striking common characteristic of all things called beautiful is their significance" (p. 23);
- (ii) "beauty is or depends upon significance" (p. 24);
- (iii) "if we are right in calling all these experiences by one name—'beautiful' or 'aesthetic'—and if expression is the essential character of one, it must be of all" (p. 124).

Of these, (i) must be false unless beauty is not itself a characteristic of objects called beautiful, but this is what Mr. Carritt uses (i) to prove; (ii) seems to set aside the distinction between meaning and concomitant that promised to be of central importance; (iii) either identifies beauty

and significance, like (ii), and the reference to "essential character" suggests this, or else assumes that any beautiful thing is beautiful only by virtue of having a second separate quality, and this always the same quality; an improbable assumption, surely.

Mr. Carritt argues that beauty is not "in the strict sense, a quality of . . . objects" because "beauty is always significant" and significance is a relation (pp. 23-24). He concludes that "it is not physical things which are properly to be called beautiful . . . we beg an important question by talking about beautiful objects" (p. 25). But to speak of "beautiful experiences" is to infringe Mr. Carritt's assertion that ordinary usage is proper usage (p. 61); and in the argument outlined above, "quality" is not used in a constant sense. First it appears as a word in ordinary language, to pose a plain question needing no elucidation; in this sense, colours, smells and beauty are all qualities. But then it is used as a technical term, to introduce one philosopher's usage, that what is determined by a relation is not a quality; and in this sense, doubtless, neither colours nor beauty are qualities. But "talking about beautiful objects" in plain language makes no philosophical assumption; it is only shown to be improper if we interpret "quality" as a plain-language term once more. Thus there is a double shift: from plain to technical sense so as to deploy the specialist premiss, then to plain again to make the conclusion important. There must, of course, be something wrong with this technical sense of "quality", because it is very doubtful whether anything does or could satisfy its requirements, and it certainly fails to leave us, in any intelligible sense, any objects to be called beautiful properly or otherwise.

Mr. Carritt's conclusion (p. 38) is "each of us has an aesthetic experience in face of a sensible object (which he then calls beautiful) . . . when it expresses to him feelings of which by his nature and past history he is capable". The distinctive sense of "express" as used here is discussed, but the account is mainly negative: expressing is not being a symptom of, not symbolising, nor stimulating, nor communicating. There are also positive suggestions, but it must be admitted that they hardly amount to a full explanation. Smiles and frowns, it is said, have a "rudimentary aesthetic expressiveness"; but this suggests that normally they are all beautiful to some degree, which is surely untrue. Elsewhere "what chiefly makes language expressive is that touch of genius . . . which deviates . . . alike from mere groans and growls and from stereotyped formulas of rage or sorrow, into expression. Then we have some degree of beauty" (p. 57). But we need to know what effect this touch of genius has on its product, in contrast to touches of other kinds. In his concluding chapter, Mr. Carritt says that in aesthetic experience we find something that expresses emotion "as words can be expressive of thought" (p. 124). At first sight this seems more illuminating; but it too is not without difficulties, for it is sometimes said that words symbolise thought, sometimes that they are the means whereby it is communicated.

Mr. Carritt's account of defective taste is ingenious: to have no taste is to have no aesthetic experiences, to have bad taste is to call beautiful what we "think true or edifying or useful or sensuously agreeable". Both of these may be found, doubtless; but do they exhaust the possibilities? Is there not also a class of persons who (to speak in plain terms) think that some ugly things are beautiful, or some beautiful things ugly? To use these plain terms may conceivably be nonsense, but the fact remains that those of whom we wish to use them are by no means those who admire or like beautiful things for non-aesthetic reasons. Similarly it might be argued that good taste, fully developed, brings not only "aesthetic

experiences" when appropriate, but also a contrary reaction in face of things that are ugly. Mr. Carritt says very little of what is ugly, and it is difficult to see how on his principles he could do so.

JOHN HOLLOWAY.

*From Statesman to Philosopher: A Study in Bolingbroke's Deism.* By WALTER MCINTOSH MERRILL. New York: Philosophical Library, 1949. Pp. 284. \$3.50.

'Who now reads Bolingbroke?' asked Burke. Who, indeed, now reads any of the English deists? The controversies concerning deism should be of some interest to contemporary philosophers, both as a smallish landmark in the history of sceptical opposition to authority and as illustrations of the scope and limits of different logical types of argument. Is Bolingbroke, then, worth reading as a serious contributor? His 'philosophy' (or blunderbuss, as Dr. Johnson called it) is curtly dismissed in Leslie Stephen's admirable study of the period. But some people apparently do read it. Professor D. G. James has recently treated Bolingbroke as a serious philosopher along with Hobbes and Locke, though only to repeat Dr. Johnson's sharp tone of partisan criticism. Mr. Merrill goes in for a moderate coat of whitewash. He concludes that Bolingbroke 'was as original as most of the deists and the most comprehensive of them all', but I do not think he proves his case. It is easy to be 'comprehensive' when a battle is over, simply by picking up the different weapons of several corpses; and the alleged 'originality' of Bolingbroke would, I think, not have suggested itself to Mr. Merrill if his meticulous examination of deist writers had been supplemented by some further knowledge of philosophers outside the deist circle. His book, which will be useful to students especially interested in Bolingbroke, is a typical piece of careful research, heavily annotated and containing a lengthy bibliography. The views of the leading deists, as well as of Bolingbroke himself, are outlined on such topics as the attributes of God, providence, miracles, the problem of evil, immortality, natural religion and ethics, essentialist metaphysics, and revelation. On other writers Mr. Merrill seems less reliable; he misrepresents Samuel Clarke's ethics on p. 27, and on p. 181 he attributes to Archbishop King passages from the *Dissertation concerning . . . Virtue or Morality* written by John Gay and first published anonymously as a preface to Law's translation of King's *Origin of Evil*. There are a few misprints, the most agreeable of which occurs on p. 1, where Bolingbroke (who died in 1751) is said to have accepted the office of Secretary of State in 1770; presumably he had by then received empirical confirmation of the immortality which he doubted in his lifetime.

D. DAICHES RAPHAEL

*F. H. Bradley.* W. F. Lofthouse, M.A., D.D. London: Epworth Press, 1949. Pp. 237. 10s. 6d.

DR. LOFTHOUSE attempts to place Bradley's thought in the history of philosophy and in its environment, to sketch its growth and structure, to confront it with its opponents and above all to show its bearing on religious questions. His book may have some value to a common reader who wants to know roughly what Bradley said, without consulting his works; but

it will not help a philosopher to understand them. It is full of nebulous misstatements ("the Kantian thing in itself, unknowable by us and only approachable by the 'modes' of time and space") proceeds largely by quotation rather than by analysis, and, when it does analyse, contains whole sections (e.g. on logical positivism) in which almost every sentence is misleading or irrelevant. Amidst all this, the singular force and originality of Bradley's mind is lost; and as for the author's religious purposes, one cannot but feel that since they are avowedly so much at odds with Bradley's, it would have been fairer to both to have treated them independently.

J. P. CORBETT.

*Philosophy for Pleasure.* By HECTOR HAWTON, with an introduction by Professor A. E. Heath. Pp. x + 214. London: Watts and Co., 1949. Price, 10s. 6d. net.

MR. HAWTON has written the sort of book which teachers of philosophy, who foresee the telling sneers of their colleagues, usually lack strong enough nerves to write. Philosophy, to Mr. Hawton, means pretty well what it means in most universities—Thales to Aristotle, and then Descartes to, let us say, Carnap. Within this range he is interested mainly in metaphysics and the various substitutes for it. He has his own views and prejudices—empiricist and naturalistic—but he tries hard to be fair to everyone and to see good in everyone, except perhaps philosophical theists, who are mocked at from time to time. Hegel and later dialecticians are taken seriously. The writer is willing to praise system-builders in terms which are not, I think, inspired only by the good manners now fashionable: Whitehead, for example, "seems to give us a glimpse of a profound wisdom which makes all the effort to follow his difficult reasoning of slight account". What Mr. Hawton means to convey by his unpromising title could be better expressed in the words "philosophy for its own sake". This brings out one of his book's merits. It is free from humbug about philosophical studies earning their keep in the welfare state. Even their claim to do good to scientists is modestly stated. There is not much analytical rigour; but philosophical beginners who read this book would gain a sense of the existence and value of analytical rigour: and there are no short ways with ancient enigmas. If philosophical beginners are to read introductory surveys, this book would serve as well as any I know. I don't think Mr. Hawton has a thesis: but he practises a little harmless mystification which helps to keep interest alive. Philosophy progresses: its progress consists of "propounding *better* questions". Questions about what? Mr. Hawton rightly does not say. But he gives the satisfying impression that, throughout the grand composition which embraces positivists and absolute idealists and sceptics and materialists, etc., there really is a single occult connecting thread, and that we can go on following it hopefully.

Mr. Hawton quotes freely, and too often without exact references. The book lists are good, and the index seems quite adequate. There is a glossary which one could pick holes in; but if one could not it would be useless to the sort of reader for whom it is intended. The only important misprint I noticed is on page 86: par. 2, line 3, for "temporarily" read "temporally". On p. 148, Stebbing's *Modern Introduction to Logic* becomes an *Introduction to Modern Logic*.

AUSTIN DUNCAN-JONES.

*Gabriel Marcel: Being and Having.* Translated by KATHARINE FARRER. Dacre Press, 1949. 10s. 6d.

THIS is a very good translation of a work which appeared in French in 1935. The major part of it is a collection of entries in M. Marcel's "metaphysical diary" during the years 1929-1933.

Of the many strands of thought in this often tortuous book I shall collect one which is central, M. Marcel's treatment of the notion of existence. He is opposed to what he calls "the philosophy of the spectator", and most traditional approaches would in his view fall under that heading. I understand him to mean something like this: these philosophies discuss our knowledge of existence in terms borrowed from those we use to describe relations holding between observer and the thing observed. But this is illegitimate, for terms useful in describing the observer's attention to a selected part of experience, are inappropriate when dealing with an all-embracing category such as Being. Others have made similar points, but unlike them M. Marcel does not draw the conclusion that all embracing categories are vacuous, vicious or "formal", but merely that they must be treated differently. Being is not something which, observer-like, we can "stand back from" because, and here we come to a characteristic Existentialist *motif*, we are "involved in it". He uses the word "incarnation" for this, and asserts that "incarnation is the basic 'given' of metaphysics". Life, in effect, is not a spectacle but a predicament. M. Marcel introduces the distinction between "problems", which are amenable to techniques and the spectator's objectivity; and "mysteries" which are not so amenable, for the above reasons. He quotes the aphorism, "Mysteries are not truths that lie beyond us; they are truths that comprehend us". It would be interesting to relate the Existentialist preoccupation with our relation to totalities in which we are involved and with the reflexive nature of consciousness, with the modern logicians' work on totalities and reflexiveness.

M. Marcel's analysis of "existence" is that to assert it of something is to say that it belongs to the same system as the body in which one is incarnate, and "is also . . . in some way united to me as my body is". Such an analysis is as egocentric as some traditional epistemological ones, but it differs from them in that the ego here is not a kind of depersonalised pure spectator but a full-blooded, not to say temperamental, personality "involved and committed" in the world. This sometimes leads to another feature of existentialist thought, that of the introduction of rather dramatic psychological concepts into epistemological or ontological discussions.

When a philosopher's general manner differs as much from the one prevalent here as M. Marcel's, it is desirable to say something about that manner as such as well as to report him within it. No doubt his thought is sometimes fanciful, and his language unusual; his reflections often have an autobiographical tinge instead of the conventional imitation of the detachment of a scientific paper, and the diary is very genuinely such in that it gives us philosophical thinking in the raw as it were, not as it is subsequently reconstructed and ordered. We see him groping his way to catch and disentangle ideas rather than presenting us with a finished product. These characteristics of exposition are indeed appropriate to his position and have the incidental consequence that even those not in sympathy with his thought will find in him a kind of material for the psychology of thinking which philosophers and psychologists seldom give us, and novelists usually give us only at a lower level.

E. A. GELLNER.

*The Problems of Philosophy.* By SATISCHANDRA CHATTERJEE, M.A., PH.D., Lecturer in Philosophy, Calcutta University. Calcutta: Das Gupta & Co. Ltd., 1949. Pp. xvi + 345. Rs. 6/8.

MUCH of this book is expository. Dr. Chatterjee gives an account, mainly for Indian students, of the varied answers of Western philosophers to some traditional problems. He discusses the subject matter and methods of philosophy, the nature and objects of knowledge, perception, truth and error, relations, universals and "thought and reality". Occasionally he indicates the views of certain schools of Indian philosophy upon these topics, stressing similarities rather than differences between Western and Indian modes of thought. On each topic he attempts to add some "positive and constructive" comments of his own.

The book summarises theories chosen from an astounding number of philosophers, good and indifferent. All are treated with equal gravity. On universals, for example, we are led through the doctrines of Plato, Russell, the Nyaya-Vaiśeṣikas, Bradley, Hoernlé, Blanshard, Cook Wilson, Stout and Kemp Smith. This list represents a fair sample of the author's erudition and his lack of selection. An Indian student may find the book more animated than I do, but if he takes the trouble to study a few of the aforementioned philosophers in the original he will appreciate the problems in no more time than it requires to read these painstaking summaries—and will find much greater force and vivacity in what is said.

Each doctrine expounded is also given some criticism, mainly in its own terms; the author tending to treat each one as good in parts, and to put these parts together to form his own theory. The result is too often an unsorted jumble of rejections and acceptances. Thus sense-data, Dr. Chatterjee thinks, are not mental; nor are they physical; nor neutral; they are "psycho-physiological" (pp. 131, 143). One is left wondering what sort of status this is. Again, relations are said not to be facts, but the ways or forms of ordering facts in a system (p. 240): yet some relations are internal and some are external (p. 244). What bearing do the two parts of this theory have on each other? Dr. Chatterjee has no clear answer. Sometimes his syntheses merely repeat the problem, as in his conclusion that universals are identities of essence, character, structure or function in particular things (p. 287). His account of perceptual knowledge apparently tries to combine the best in realism, idealism, and a doctrine that I can only call verbalism. Surely it is carrying reconciliation too far to assert that there is a "verbal element" in the constitution of a rose or an orange (p. 98)?

Dr Chatterjee believes that philosophy is neither a kind of science nor the logic of science. It is a "metaphysics of reality beyond sensuous phenomena" (p. 9). Moral, aesthetic and religious values belong to "reality", as do things-in-themselves (adopted by Dr. Chatterjee in all but name). Philosophy is said to rationalise the experience of such realities. The most direct experience is a "pure existence-consciousness", which yet is not consciousness of any particular form of existence. This experience is also said to be "real in the highest sense" or "the transcendent, absolute reality" (pp. 333-4). It is hard to see what philosophy can rationalise here.

G. P. HENDERSON.

*Dreadful Freedom. A Critique of Existentialism.* By MARJORIE GRENE. University of Chicago Press (London : Cambridge University Press), 1948. Pp. ix + 149. Price 15s.

This is a critical discussion of existentialism, throughout intelligible and illuminating. Apart from an account of the political theory of French existentialism, there are chapters on Kierkegaard, Sartre and Heidegger, whose views, though themselves criticized, are favourably contrasted with those of Jaspers and Marcel. The doctrines discussed are the startling psychological and ethical pronouncements whereby the philosophy of existence has become fashionable and a challenge to customary culture.

Mrs. Grene begins with a definition of existentialism as 'the philosophy which declares as its first principle that existence is prior to essence'. And by existence, she aptly points out, the existentialist does not mean the kind of fact accepted as primitive by pragmatists or logical positivists, but a 'particular human fact'. The problem now, as the author sees it, is how existentialists can generate values, particularly concerning the relation between persons, out of the bedrock of a particular human fact, be it 'inwardness' as with Kierkegaard, or 'freedom' as with Sartre. In the 'Postscript', the problem is given a general answer to the effect that existentialism is an 'ethic of integrity', though not a very hopeful one.

Throughout, the discussion is conducted in face of Sartre's and Heidegger's declarations (to which Mrs. Grene refers, p. 47) that their philosophy is not primarily concerned with ethical problems but with a new ontology. This the author prefers not to expound. With that she fails to explain the problem of transcendence and the phenomenological approach in existentialist speculation. Yet these parts of theory carry, if not conviction, at least some weight as premises from which, rather than from 'dread' or any such particular human fact, existentialism derives its logic and all its notions. Just because these latter appear faulty or perplexing, a study of their derivation would seem to be called for. To rise to the level of the 'meta-problematical', as Marcel has called it, beyond the distinction between a subject asserting being and being thus asserted, a certain kind of thought is required : thought at one remove. If this had been part of the author's performance, she might have established closer contact with the aims and fundamentals of existentialist metaphysics. Her attempt at criticism would then have become more subtle and more complete.

The bibliographical note appended to the book is, even for the purposes of an introduction, somewhat scanty and haphazard. On pp. 39 and 148, Kant's practical imperative is quoted imperfectly. A misprint occurs in l. 18, p. 139.

W. VON LEYDEN.

*The Crisis of the Human Person.* By J. B. COATES. London : Longmans, Green & Co. Pp. 256. 12s. 6d.

"PERSONALISM", according to Mr Coates, is "the name given to a number of philosophies which correlate the conceptions of personality and value, which conceive of personality as a unique entity in every human being which has a movement towards value and is the source of our knowledge of value". In such light as this definition affords, he examines the views of a number of well-known writers (Berdyaev, Buber, Mumford, Mannheim, Burnham, Laski, Aldous Huxley, C. S. Lewis and others)

who may be described as in varying degrees exponents of, sympathisers with, deviationists from and possible (if rather unlikely) candidates for redemption to, the personalist point of view. There are, besides, introductory and terminal essays devoted to explaining what Personalism is all about, and to commanding it as a social philosophy for those who, believing the old orthodoxy of liberal individualism to be bankrupt, are yet unwilling to see a marxist or fascist collectivism installed as its Official Receiver. Mr Coates follows Berdyaev and Buber in asserting, on intuitive grounds, the autonomy (Spirituality, Freedom, Creativity, etc.) of the person over against materialism, "scientism", the State and other institutions (including churches), while insisting at the same time that personality involves an organic relation to other persons and to society at large : this is to part company with individualism and with some forms of existentialism, and also accounts for the notably ambivalent attitude towards Marxism of some of the continental personalists. The imputation of subjectivity to which such a theory would appear vulnerable, is evaded by maintaining that all persons are united in a common relationship to God, or (for the benefit of "humanists"), "objective values" ; and the political outcome seems to be an optimistic form of devolutionary socialism.

The terminology of the personalist writers makes it difficult to determine how much in them is *afflatus*, how much merely *flatus* ; Mr Coates, unfortunately, admires his heroes too much to want to criticise them, so that, like them, he leaves the reader uncertain whether he is expressing an attitude, propounding a theory, or imparting a revelation. But his treatment has the merits of being succinct and tolerably clear, and may be recommended as a useful introduction to its subject.

P. L. HEATH.

*The Arts and their Interrelations.* By THOMAS MUNRO. New York : The Liberal Arts Press, 1949. Pp. xv + 559. \$7.50.

THIS is a very useful book to any student of aesthetics. 'Art' is one of the prize words of our culture, highly charged with emotion but extremely vague in content. Its use cannot be fully understood apart from its history and in relation to the heterogeneous collection of objects to which it is, and has been, applied. Their number and variety is too often overlooked by aesthetic philosophers. Very little can have been overlooked by Professor Munro. He has given a comprehensive survey of the arts, their products, media, techniques, and creative processes and of the definitions and conceptions by which they have at different times been judged and classified. The result is not a history but, rather, an historical anatomy of art. Something of the sort is certainly required as a preliminary to a study of art and aesthetics and I know of no other work in English which provides it with comparable thoroughness. His aim has been to give an up-to-date, scientific 'dictionary' definition of 'Art' by methods purely historical and descriptive and this is given on p. 107. It is, naturally, too long to quote. This is allied to suggestions for modern classifications of the arts for different purposes. As the best for an all-round multi-purpose classification he suggests *sense primarily addressed*, visual, auditory, etc. This is not very revolutionary, and, of course, as he realises, breaks down for literature which must form a separate category. However, most of his suggestions are sensible and practical. One realises that he is writing not only for theorists but also for curators, teachers, librarians. Its combination of the theoretical and practical is, indeed, a refreshing

feature of the book. No doubt specialists in its various sections may quarrel with details, but to have made such a survey, within reasonable compass, without becoming unbearably superficial, is a considerable achievement. One would like a similar survey for that other eulogistic term 'Science'.

I should, however, criticise the first, or philosophical, part of Professor Munro's definition on the grounds that although proffered as objective and descriptive, it is in fact expressed in terms of the view that art is the expression and communication of emotion and particularly of aesthetic emotion or experience. This seems to be accepted without adequate analysis and there is no recognition of alternative 'objective' definitions, whatever their merits or demerits. The name of Alexander, *e.g.* does not appear. Professor Munro eschews all dealings with evaluation, but like so many other pure descriptionists in fact assumes a value attitude which he does not criticise. His, I suspect, is the rather simple subjectivist view that what is felt to be good by someone *is* good. It is, therefore, further unfortunate that he has taken no account of the establishment of value judgments in modern emotive theories, but this, perhaps, is not surprising since they have not yet been adequately applied to aesthetics, though more account might have been taken of the work of L. A. Richards. Moreover, the definition is framed in terms of the question-begging words 'use' and 'intention' *e.g.* "Art is skill in making or doing that which is *used* or *intended*<sup>1</sup> as a stimulus to satisfactory aesthetic experience. . . ." The reasons he suggests on p. 90 when discussing another definition is that this mode of expression is much more cautious and objective since although there may be endless dispute about what a work of art is, or does, there can be no doubt whatever about what the artist *intended* it to be or do. This seems to me an astonishing statement. Professor Munro gives no hint about how he would obtain this valuable information. By questionnaire? But how, if the artist is inarticulate—or dead? Whether we judge the artist's intention in judging his work, is doubtful, but it is certain that to discover it is far less easy than to determine the quality of the work. However, there is much to be grateful for in Professor Munro's ample volume.

MARGARET MACDONALD.

*The Values of Life.* By E. J. URWICK. Edited with an Introductory Essay on his Social Philosophy by John A. Irving. Toronto University Press (London : Geoffrey Cumberlege). Pp. lxv + 244. 20s.

WE are irresistably reminded of Cephalus by the serenity and detachment of this posthumous collection of essays by a social philosopher best known in this country for his *Philosophy and Social Progress*. No one is likely to be provoked by a castigation of capitalism ending "it cannot be beyond our ingenuity to devise something less shockingly wasteful" (p. 117), nor is there much promise of a lively ethical controversy in the proposal to settle, by an appeal to Aristotle's "really good man", the problem of deciding by what authority we say that the conduct-determining values of the Borneo head-hunter are less good than those of a kindly American (p. 34). The bulk of the essays are on love, friendship, simplicity, beauty, wealth, progress, knowledge and truth, and happiness. Some of these "ideals" are "real", some not. The unifying theme is an assumption of selflessness as the test of worthwhileness of any given end. It is odd

<sup>1</sup> My italics.

to find "progress" among the "real" values; odder still to read that it occurs "whenever change opens wider the door . . . to the means of living a fuller and better life" (p. 131), as though it were one thing to value *a*, *b* and *c* (say love, beauty and truth) and another thing to value *d*, which turns out to be the attainment of *a*, *b* and *c*. Professor Urwick hasn't seen that the hedonistic paradox applies, *mutatis mutandis*, to the concept of progress as a value. In fact, he wants to make good the repudiation of religious sanctions of morality by elevating Progress as an alternative authority "to make absolute the principles of conduct to which we want to be loyal" (p. 142). This is hypostatisation with a vengeance. But it would be ungenerous to cavil at the logic behind the sermon. This is not a philosophical treatise, but, rather, a mild exhortation, intensely personal, entirely lucid and full of practical wisdom, such as advice on how to find opportunities for the exercise of true charity in the modern world.

It would have been better if the editor had not included the essays on *Capitalism and Value* and *Labour and Value*. The plea for an intelligent "Plenty economy" to supersede a wasteful "scarcity economy" wasn't written in times of a U.N. Food and Agricultural Organisation. A curious error of fact occurs on pp. 11, 25 and 116, where a version of Hume's dictum on reason as the slave of the passions is fathered on Hobbes.

B. MAYO.

*Philosophy for the Future*. Edited by R. W. SELLARS, V. J. MCGILL and MARVIN FARBER. (New York: The Macmillan Company, 1949) Pp. xii + 657. Price \$ (U.S.) 7.50.

THE philosophy for the future is dialectical materialism and these twenty-eight essays by as many philosophers, scientists and historians are designed to explore and reformulate this philosophy. Materialism, say the editors, "emphasises explanation in terms of causal and genetic relations, in contrast to idealism which makes the relations of ideas primary and explains the course of the world in terms of abstractions". Such a formulation of the programme is, of course, philosophically naive. Is not all explanation in terms of abstractions? And if all genetic explanation is materialistic, materialism has lost much of its sting. Some of the authors appear quite unaware of the philosophical problems involved and, in particular, of the reasons which led men to embracing idealism. Thus it is amusing to find this: "The world is in eternal flux, so that two things are never alike and not even a single thing remains the same. Yet, it is possible to separate special aspects of this world and to treat them as if they do not change. Professor Levy has called these aspects 'isolates', and isolates can stay invariant and become an object of discourse". Professor Levy may call them isolates, but Plato called them Ideas; and Plato was not an Idealist, because his isolates had something called 'objective reality', and the mark of the idealist beast is to deny objective reality to such things.

Nevertheless, there is a clear and consistent doctrine running through these essays. The authors believe that every man must have a "philosophy" and that his doings and sufferings, to say nothing of the doings and sufferings of society, will depend on the philosophy that he adopts. There is a tendency among some philosophers to-day to treat philosophical theories as technical devices for solving technical problems and these essayists do well, in my opinion, to protest against this. The core of their

materialism is a suspicion of 'wholly new principles' or 'mystic agencies'. They are on the side of empiricism and science against supernaturalism and superstition.

It is a pity, therefore, that their tone and method of argument should be so strongly reminiscent of the witch-hunt. Mr. Hill, indeed, praises Hobbes for using logic and rational demonstration instead of scriptural citation; but for his colleagues doctrinal orthodoxy is the overriding consideration. To take but one example: Mr. Judd Marmor, in an otherwise penetrating essay on psycho-analysis, is at pains to prove (a) that psycho-analysis "stripped of its debatable superstructure and reduced to its basic concepts is in accord with the principles of modern materialism" and (b) that Freud's error was not Idealism but Mechanistic Materialism. These are the arguments of the advocate in a heresy trial, not the arguments of a philosopher or a scientist. And, in general, the writers are interested not in the truth of a rival theory or the cogency of the arguments by which it is supported, but in the social origins of its author, its probable effects on society and, above all, its ability to be squared with the Scriptures. It is, of course, true that metaphysical presuppositions affect moral codes and that the human race has been put to a vast deal of unnecessary suffering because, for religious reasons, men have been prohibited from rationally investigating the causes of their suffering. But these are arguments against supernaturalism, not arguments for materialism, and it is a pity that the materialists should not know their own friends. Thus Voltaire, who probably did more than any man to free us from superstitions, is only mentioned once and then only to be condemned for having opposed the French Materialists.

Of the philosophical essays Mr. Cornforth's is perhaps the most lucid, Mr. Wilfred Sellars' certainly the most profound. Mr. Cornforth tries to show that modern logical empiricism is nothing but subjective idealism in disguise. He has no difficulty in tarring Wittgenstein's *Tractatus* with the idealist brush and he traces skilfully the stages by which logical empiricists have been forced to bring back all the old metaphysical problems which they claimed to have discarded. But he is on less solid ground when he argues that the metaphysic to which logical empiricism must lead is an idealistic one, for there is no reason why the logical empiricist should ignore, (although they have for the most part ignored), the social and economic origins of scientific concepts.

Mr. Sellars traces the scholastic conception of 'substantial form' to a faulty analysis of Aristotle's "thing-nature" philosophy, which he contrasts with an "event-law" philosophy. The latter, he says is fundamental to the former; for the nature or dispositions of a thing must ultimately be defined in terms of its specific states in specific circumstances. If we fail to see this we are apt to suppose that such dispositions are entities which generate or produce states  $S_1 \dots S_n$  in circumstances  $C_1 \dots C_n$  and to find here "an ontological fruitfulness, an overflow, a necessity which is no mere consequence of definition". Once we are saddled with substantial forms as the causes of the behaviour of things we are saddled also with the "abstractive theory of scientific knowledge", according to which "a truly scientific understanding of the various kinds of object in the world of nature is achieved by abstracting the forms of these objects from experienced cases, just as (supposedly) a truly scientific understanding of geometrical shapes is achieved by abstracting geometrical natures from experienced cases". Since the methods of modern science are patently quite different from this the modern Thomists have been forced

to believe in two types of science, a phenomenalist science which is all that we poor mortals can achieve about the physical world and an angelic science which still grasps substantial forms, but which we can only understand as applied to ourselves. Mr. Sellars exposes the fallacy underlying all this, but I cannot help thinking that his orthodoxy is in danger.

There are two conspicuous deficiencies in this book. The first is an adequate critique of pragmatism, the second and more important a positive account of dialectical materialism. We learn much of the errors of others, but we are not told precisely how materialism surmounts the problems which its rivals have tried (in vain) to solve.

P. H. NOWELL-SMITH.

*Der Begriff als psychisches Erlebnis.* By GUSTAV LEBZELTERN. Graz-Wien: Buchverlag vormals Leykam, 1946. Pp. 91.

*Der Syllogismus psychologisch betrachtet.* By GUSTAV LEBZELTERN. Graz-Wien: Leykam-Verlag, 1948. Pp. 86.

These two small books are interesting attempts to sketch the psychology of logical processes: their author is a pupil of Bühler and Lindworsky, and is carrying on their investigations. The first book deals with the various ways in which concepts function in experience, from the stage at which they are clearly conscious, and associated with illustrative images, through the stage at which such images dwindle away completely, on to the final stage where we behave and talk appropriately, and in this sense *have* a notion, though it is quite unrepresented in experience. What was at first clearly *bewusst*, becomes less and less *bewusst*, until it is merely *gewusst*. The author also has interesting things to say about the experienced differences between an image used illustratively and the same image used conceptually. And he also classifies our *non-imaginal* conceptual experiences into *Übersichtserlebnisse*, in which it is as if there were a vast amount of detail spread out before us for review, *Hinwendungserlebnisse*, in which we merely seem to turn towards something without having it spread before us in this manner, and *Ruckerlebnisse* in which we have the painful sense of dragging forth details, meanings, etc., which have grown strange in unconsciousness. The second book analyses our syllogistic reasonings and tries to show how their actual pattern differs from the model set up by tradition. It puts forward a view of the actual import of particular judgments which differs both from the traditional and the modern accounts, and it shows how this view bears upon the interpretation and classification of various syllogistic moods (e.g. *Darii*). I am not convinced that our normal thought-processes conform entirely to the models set up by Herr Lebzeltner, but even less do they conform to the traditional and modern formalisations. And I think his attempt to discover how we actually do think is praiseworthy and illuminating.

J. N. FINDLAY.

*Barbara Celarent: A Description of Scholastic Dialectic.* By THOMAS GILBY, O.P. London: Longmans, Green and Co., 1949. Pp. xiii + 303. 18s.

THIS breezy book, written by a Dominican friar between action stations on H.M.S. *Renown* (as the preface tells us), is one which the author obviously

enjoyed writing, and his enjoyment is infectious enough to be communicated to the reader. It is not what a perusal of the chapter-headings would suggest—a text-book of Traditional Logic. Nor is it what the publishers call it on the back of the dust-cover—an "account of Thomist philosophy". It is more what the author calls it in his preface—"a free commentary on the traditional logic". (His description of it as "this first volume" suggests that more are meant to follow, dealing with the later subjects of the standard Thomist course in the same way.) Its message is that there are no cobwebs on Scholasticism and that St. Thomas was a whacking fine fellow. The public envisaged seems to be one which was put through elementary Scholastic Logic at school, and can still understand small scraps of Mediaeval Latin, and is now in a mood to be reminded of those far-off, half-forgotten things, so long as no great further intellectual effort is demanded.

The chapters are short. To the non-Catholic philosophical reader the most interesting will probably be the one which gives an account of formal syllogistic disputation as still practised in Dominican schools. Incidentally, from the example there used he will learn something of the complexities of the Dominican theory of Free Will, which may make him wonder whether our vulgar Protestant or Secularist treatment of that subject may not sometimes be rather slapdash.

A. M. MACIVER.

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#### NOTES

The third German Congress of Philosophy will be held at Bremen from the 2nd to the 6th October, 1950. The discussions will be organised on a Symposium basis and the subjects provisionally arranged for the Symposia are :

Might and Right.

Historical Materialism and the Problem of Ideology.

Philosophical Presuppositions of Logistic.

The Problem of the External World.

Problems of Natural Philosophy.

The Problem of Mythical Consciousness, Language and Poetry.

Existentialism.

The Organising Secretary of the Congress is Dr. Wein of the University of Bremen. The Organising Committee is anxious that philosophers from other countries should attend and would welcome even provisional enrollments to be made at an early date.

In 1901 F. C. S. Schiller, under the pseudonym of A. Troglodyte, produced a burlesque number of *MIND* entitled *MIND!* This did not of course belong to the *MIND* series, though it was got up to look like numbers of this series. Copies of this burlesque number have of recent years been hard to procure, but the publishers of *MIND* (Messrs. Thomas Nelson & Sons, Ltd., Parkside Works, Edinburgh 9) have discovered a considerable parcel of undistributed copies which they are prepared to dispose of at 2s. 6d. each. Requests for these should be sent direct to Messrs. Nelson and not to the Editor of *MIND*.

#### ERRATUM

In P. Geach's Discussion Note 'On Rigour in Semantics' (*MIND*, January, 1950), on page 520, line 30, for 'consistent' read 'inconsistent'.

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